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ORIGINAL LECTURES.

PSEUDO-HYPERTROPHIC MUSCULAR PARALYSIS.

A Clinical Lecture delivered in Rush Medical College, Chicago.

BY NORMAN BRIDGE, M.D.,

PROFESSOR OF HYGIENE, AND ADJUNCT PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE.

GENTLEMEN: The lad who is before you has a curious disease. As he walks in, you observe that he has a waddling gait, and seems to be lame on both sides; he lifts his feet as if they were weighted, and carries them far apart, and as he steps he throws the foot forward slowly and with difficulty, and his step is short. As he sits here before you, he would hardly be supposed to be sick. He looks a little pale from having been much indoors; his face is thin, and his large head and broad forehead give him an appearance of mental strength that does not exist. His peculiar movements as he walks in are the result of weakness; all his muscles are weak, and his movements indicate this fact. He sits easily in his chair, but when he attempts to get up he is unable to do so in the usual manner. He has not strength to rise directly from his seat as one usually does, but he awkwardly turns his body round before rising, faces backward, and by the aid of his hands, resting them first on the seat and then on the back of the chair, he helps himself to rise to a standing posture. In executing the manoeuvre he uses, all fours, and if he should attempt to rise from the recumbent posture he would be obliged to turn first to the prone position and, after rising to a position on his hands and knees, to help himself up to a standing posture by means of his hands against his knees and thighs. As he reaches the perpendicular, you discover that he first staggers and shakes slightly till he gets the grasp of his equilibrium, when he slowly walks off with the peculiar gait. In going up stairs he is obliged to use all fours, creeping from step to step.

As he stands, you see that his shoulders are thrown far back, and the lumbar region of the spine is deeply arched; the spinal column is in a state of lordosis. This curvature is seen to be even more marked when the clothing is removed. The lordosis is present only when standing—as the patient sits it disappears entirely. On stripping the patient another feature is noticeable, namely, an attenuation of the muscles over portions of the body. Not only is there little adipose tissue, but the muscles seem to be even thinner and less developed than normal. Possibly over the pectoral region and over the area of the latissimus dorsi muscles the flatness is greater than over the general surface, but the difference is not very marked. The calves of the legs are an exception to this general appearance of muscular poverty; they are actually enlarged beyond their normal size. We are told that they were formerly

much larger than they are now; so large, indeed, that they were the subject of remark by all who saw them, and that medical advice was sought on account of this unusual appearance.

The boy is ten years old, and, his mother tells us, has never had any acute sickness, except an occasional slight sore throat. He was tardy in learning to creep and to walk, and was always weak in his legs. He early—probably by or before his fifth year—showed the lordosis of the spine, but since his sixth year this symptom has steadily grown worse, and the weakness has also steadily increased. For half his lifetime he has been obliged to rise from his chair and to go up stairs in the way I have described. The calves bulged markedly till within two years—latterly they have become less prominent. Slight exercise causes fatigue, and on becoming chilled he often complains of aching of his thighs and back. He has dyspepsia, and often passes undigested food by the bowels, and his extremities are often discovered to be cold. Otherwise he seems to be in fair general health. He acts and talks in a shy manner, and like a child somewhat younger. He is very quiet, and does not attempt to join his playfellows in any active sport.

This clinical picture is that, almost unmistakably, of one disease. So far as is now known, no other disorder much resembles it. It is pseudo-hypertrophic muscular paralysis, and is so named—or receives its qualifying designation—from the apparent hypertrophy of the muscles, as we see in the calves of this patient.

In this disease the muscles actually waste, while there occurs an increase in the connective tissue and fat about and among them. Some of the muscular fibres undergo degeneration and apparently become connective tissue. The tissues surrounding the muscular fibres undergo hypertrophy, the muscular tissue wastes (from pressure, probably), and the paralysis is the paralysis of weakness. Nearly all the muscles waste, the pectoral and the latissimus dorsi perhaps most, but the hypertrophy is not uniform over the body. The bulging occurs most in the calves, as in this case, but may occur at the seat of the masses of muscle in various parts of the body, giving a false appearance of great muscular development. Some cases, unlike this one, have slight talipes, the heels being raised, from contractures in the muscles connected with the tendo Achillis. The disease invades the muscles of the heart, and causes death from weakness or paralysis of this organ. The invasion of the muscles is general over the body, and so, while it is a muscular atrophy, it differs in this particular from the so-called progressive muscular atrophy. In the latter disease the several muscles are attacked in succession, those of the upper extremity first, and the muscles that are spared remain in their integrity for years after the disease begins. Moreover, in the progressive muscular atrophy there is no pseudo-hypertrophy, and the disease usually occurs in adults.

The pseudo-hypertrophic muscular paralysis begin

in childhood, attacks the muscles of the body generally, slowly and progressively increasing, usually without a halt, till death ensues. This event usually occurs by or before the period of puberty is reached. If the disease ceases its progress for a time, it does not disappear nor much improve, but after a period of quiescence it marches on to a fatal termination. It occurs most often in boys. In a portion of the cases there is a lesion of the spinal cord, probably consisting in an inflammatory change in the gray matter. The anterior cornua and the lateral portions of the gray matter have been found, post mortem, in a condition of disorganization. What relation the changes in the muscles bear to those in the cord nobody knows; which of these lesions precedes the other in point of time is not known, and all calculation as to whether the disease begins in the cord and leads to the changes in the muscles, or *vice versa*, is unsettled by the fact that in some of the cases careful post-mortem examinations have failed to discover any lesion of the cord of any kind or degree. Possibly more searching methods may in the future demonstrate that lesions of the cord are invariable. The clinical fact of significance to us is that in this affection the lesion of the muscles always exists; of that we are certain. That there is always lesion of the cord, gross or minute, is perhaps true.

The mind is usually unaffected except that it is weak, in some cases very weak. A few of the children late in the disease pass into a condition of idiocy. This is an event that should not surprise us when we consider the spinal lesions which sometimes exist and when we reflect that such changes may easily extend to the brain.

There has been some speculation as to the mechanism of the lordosis. It has been attributed to weakness of the extensor muscles of the spine, but on this theory the phenomenon ought not to disappear in the sitting posture. A better explanation is this: that as the patient stands the pelvis is unnaturally flexed upon the thighs owing to weakness of the extensor muscles that counteract this tendency; it is tipped or inclined forwards and the deep curvature of the spine is simply in compensation for this and to enable the patient to maintain the erect posture and his equilibrium.

There ought to be little danger of confusing this disease with any other. A few symptoms are always present, and, appearing together, they are diagnostic. Yet the disease has been mistaken for some affection of the spinal bones, for disease of the hip-joints and partial paralysis of the lower extremities. In a child with lordosis of the spine, a waddling, unsteady gait with the feet far apart, and the peculiar manœuvre on rising from the sitting and recumbent postures, if there is bulging of the calves with or without the same feature in other parts of the body, you may say surely that it is a case of this disease. These few elements are always present in greater or less degree: the youth of the patient; the lordosis; the characteristic gait; the peculiar manner of rising to the standing position; bulging over of certain muscles and the general weakness. Not only are all these present in this disease, but they are not present together in any other affection whatsoever.

The prognosis is grave indeed, and must so remain till more is known of the etiology and proper treatment of the disease. So far the patients all die before they

reach adult life. Death may occur from exhaustion—which means, chiefly, heart exhaustion—and it may result from some intercurrent disease.

This peculiar affection is to a certain, probably we ought to say to a slight, extent hereditary. It would be more properly called a diathetic disorder that may be peculiar to a family. Certain families have shown a predisposition to it and have suffered its occurrence in several successive generations.

The treatment is theoretical and experimental. No treatment has had any particular influence over the disease. Various tonics, phosphorus and arsenic, and electricity, have been tried, with slight or no effect on the progress of the malady. As the weakness rapidly increases when the patient ceases to exercise, it is desirable to keep up some vigor of the muscles as long as possible by exercise and manipulation. In the present case some elixir of calisaya with nux vomica has been given during the past week for the dyspepsia, with the effect of partially removing it. But we cannot look for any improvement in the condition of the muscles and connective tissue from this treatment. The improved digestion may enable the patient to maintain his strength a little longer and postpone a little the day of dissolution, but I have no faith that anything more will be accomplished by it. To facilitate the general nutrition daily inunctions of oil over the entire body, with rubbing, have been ordered.¹

ORIGINAL ARTICLES.

THE GERMICIDE POWER OF POTASSIUM PERMANGANATE.

BY GEORGE M. STERNBERG, M.D.,
MAJOR AND SURGEON, U. S. ARMY.

In the very instructive paper by Prof. Bartholow on "Permanganate of Potassium: its Action and Uses," published in *THE MEDICAL NEWS* of November 22, 1884, the following reference is made to my experiments relating to the germicide action of this agent:

"Most of the experiments made have been with microbes contained in culture-fluids; for example, the very able and elaborate research made by Dr. Sternberg.² Under these circumstances, the results are better than can be achieved from the use of the same antiseptics in disease, *ceteris paribus*."

This statement is undoubtedly correct, and is especially true as regards an agent like potassium permanganate, which is very quickly decomposed in the presence of organic matter. It is for this reason that in the experimental research referred to by

¹ This case was re-examined four months after its presentation at the clinic. It was found that the boy had added perceptibly to his stature; that he had been in better vigor than for several years; that no dyspepsia had occurred and that he was able to move about quite as well, possibly slightly better, than at his former visit. There had developed slight talipes varus and the bulging of the calves had decreased. The treatment had been continuously kept up, both as to the medicine and the inunctions and manipulations. The material used for the inunctions was, most of the time, olive oil, and it had been applied each day once.

² Experiments to Determine the Germicide Value of Therapeutic Agents. *Amer. Journ. Med. Sciences*, April, 1883.

Prof. Bartholow, which had special reference to therapeutic possibilities, the writer dropped the permanganate from the list of substances to be studied from this point of view, after having determined that it had very decided germicide power. It destroyed the vitality of the micrococcus of pus in the proportion of 1 to 833.

This germicide power cannot, however, be made available for the destruction of germs in the blood or tissues for the reason that the permanganate is itself quickly decomposed by contact with organic matter. This point will be referred to later, and some recent experiments will be reported which show that, under the conditions mentioned in my paper above referred to, this agent does destroy germs in the proportion given. Prof. Bartholow continues:

"Sternberg assigns to bichloride of mercury (mercuric chloride) the first place as an antiseptic; yet he puts permanganate of potassium second."

There is a mistake here which seems to result from the use, by Prof. Bartholow, of the terms germicide and antiseptic as synonymous. Mercuric chloride does appear at the head of the list both of germicides (Table No. III.) and of antiseptics (Table No. IV.); but potassium permanganate only appears in Tables Nos. I. and II., which relate to the *germicide* power of the agents named, as proved by their power to destroy the vitality of a single test organism—a micrococcus.

For the reason already stated, the permanganate was dropped, after this preliminary trial, from the list of *therapeutic* agents which the writer proposed to test with reference to their *germicide* power. As I consider the distinction between germicides and antiseptics to be a very important one, I take the liberty of introducing here the definition which I have given in the chapter on "Germicides and Antiseptics" in my work on *Bacteria*.

"By a *germicide* agent we mean one which has the power to destroy the vitality of the various species of bacteria known to us, including those disease-germs which have been demonstrated, such as the anthrax bacillus, the bacterium of symptomatic anthrax, the micrococcus of fowl cholera, that of septicæmia in the rabbit, etc. *Germicides* are also *antiseptics*, as the bacteria of putrefaction are killed by them as well as those mentioned. They may also arrest putrefactive decomposition in quantities less than are required to destroy completely putrefactive organisms. But an antiseptic is not necessarily a germicide, for experiment proves that certain substances arrest putrefaction which have not the power to kill the bacteria to which this is due. This they do by arresting the vital activity—multiplication—of the germs of putrefaction, or by so changing the nutritive pabulum required for the development of these germs that they are unable to appropriate it to their use."

As examples of antiseptics which are not germicides—except in the strongest solutions and after very long exposure—we may mention chloroform and alcohol, which inhibit, so to speak, the development of germs; and the metallic salts, such as sulphate of iron, sulphate of zinc, etc., which prevent

the multiplication of germs by precipitating the albuminous material in fluid media which would otherwise furnish abundant pabulum for the development of putrefactive bacteria.

Having thus made clear the distinction which the progress of our knowledge in this direction has made it necessary to establish, with reference to the agents which were formerly all classed together as "antiseptics"—a distinction which alone justifies the use of the term *germicide*—I would remark that permanganate of potassium occupies the second place in my Table No. II., not because it ranks next to mercuric chloride among all known germicide agents, but because it has this relative position in the limited list which I selected, for special reasons, for the experimental research, the results of which are reported in the paper referred to.

I may be permitted to remark, *en passant*, that experiments of this nature take a great amount of time, and that the only reason why a larger list of chemical agents was not included in the above-mentioned research was that the writer had no assistance and was obliged to restrict his experimental work within the limits imposed by his own available time and pecuniary resources. Fortunately, circumstances are at present more favorable for the prosecution of work of this character, and I am now engaged in an extended research upon the "comparative value of disinfectants," which I have undertaken in conformity with the wishes of the American Public Health Association, and with the substantial assistance of several of the most active State Boards of Health. This pecuniary aid has enabled the committee, of which I am Chairman, to secure the services of two very competent assistants, who are now actively engaged in the work under my direction and with all of the personal assistance which other duties enable me to render. It has been decided by the committee that the results of our investigations shall be published from time to time in some leading medical or sanitary periodical, in advance of our complete report to the Public Health Association, at its next annual meeting, in order that they may be utilized by health officials and by the public in case of an epidemic of cholera next summer.

I proceed to quote from Prof. Bartholow's article, which, with due respect, I have taken as a text in the present communication, with a view to showing how many elements enter into the equation when the statement is made that a given agent is a germicide in a given proportion. Dr. Bartholow says:

"Whilst bichloride is effective in the proportion of 1 part to 20,000 of fluid, the permanganate of potassium acts to the same degree in the proportion of 1 part to 833 of solution."

Now, it must be remembered that these results relate to a single test-organism, and that the time of exposure adopted was two hours. They cannot, therefore, be compared directly with other experiments made by a different method, upon different organisms, and with a different time of exposure. All of the results reported by different authors, even by those who have been the most careful and exact in the application of the tests adopted, must be looked upon as approxi-

mate, and not as exactly representing the germicide power of the agent named. Thus in my experiments with permanganate of potash, I commenced with a four per cent. solution, and, finding this effectual, sought to ascertain the limit of germicide power by diluting my standard solution after each experiment with an equal quantity of distilled water. This gave me the following series of experiments, in which no development of the test-organism occurred in a sterile culture-solution inoculated with the germs—micrococci—after two hours' exposure to the germicide agent, viz.: 4 per cent., 2 per cent., 1 per cent., 0.5 per cent., 0.25 per cent., 0.12 per cent. (last experiment in duplicate). Upon again diluting the standard solution one-half (making 0.06 per cent.), it failed to destroy the vitality of the micrococci, which multiplied freely in a sterile culture-solution in the presence of this amount. Here, then, we have the line between success and failure drawn between the limits 0.12 per cent. = 1:833, and 0.06 per cent. = 1:1666. To establish the exact limits for the germ in question, it would be necessary to try numerous experiments between these limits, and not only to exercise the greatest care in weighing or measuring the standard solution, but to operate each time upon the same number of germs contained in a uniform quantity of culture-fluid of uniform composition. These conditions might possibly be fulfilled, but from a practical point of view it is hardly necessary to insist upon their being fulfilled, inasmuch as the limits established are sufficiently accurate to serve as a guide for the use of this agent in disinfection. But in applying the experimental data here given, it must be remembered that the permanganate itself is destroyed by contact with organic matter, and that the destruction of micrococci in a drop of culture-fluid is a very different thing from the destruction of the same microorganisms in a large amount of organic material, as, for example, in the blood of an animal, or in liquid feces, by which the permanganate in dilute solution would be almost instantly decomposed.

In the writer's experiments, made in Baltimore in 1881,¹ it was found that 1 part in 50 was required to destroy the septic micrococcus in the blood of a rabbit just dead from induced septicæmia, the test of disinfection being inoculation of healthy rabbits with the disinfected material. According to Arloing, Connevin, and Thomas, a five per cent. solution destroys the fresh virus of symptomatic anthrax, but has no effect upon the dried virus. These statements are not at all in conflict with the results reported in my experiments heretofore referred to, and these results are substantiated by the following recent experiments made by the same method:

Nov. 26, 1884, a pure culture of the micrococcus of pus was subjected to the action of potassium permanganate for two hours in the proportion of 1 part to 500, and in the proportion of 1 part to 1000. Four culture-tubes containing a sterilized solution of beef-peptone were inoculated with the micrococci thus exposed (it is my practice to make

every experiment in duplicate), and were placed in a culture-oven maintained at 38° C. (100.4° F.) for forty-eight hours. No development occurred in either of the tubes.

On the 29th of November, a similar experiment was made with a culture-solution containing both micrococci and bacilli. In this experiment there was no development of the micrococci, but the bacilli developed abundantly after exposure to the 1 to 1000 solution. No development of bacilli (*B. subtilis*) occurred, however, after exposure to 1 part in 250. In these experiments the permanganate, although in dilute solution, was not neutralized by the small amount of organic material contained in the drop of culture-fluid exposed to the action of the germicide agent. In the following experiments the conditions were varied, and a larger proportion of the permanganate failed to exert any germicide power.

Nov. 24th, equal parts of a 0.4 per cent. solution (1:250) of potassium permanganate and of "broken-down" beef-tea were mixed in a germ-proof receptacle, and allowed to stand for two hours. Two culture-tubes were then inoculated with a minute drop of the mixture, and were placed in the oven. At the end of twenty-four hours, an abundant development of putrefactive bacteria had taken place. In this experiment, then, we have a failure in the proportion of 1:500, but the experiment does not in the least invalidate those previously reported. The truth is, that in making the above mixture the permanganate is almost instantly decomposed by the excess of organic matter, while in the experiments in which a single drop of culture-fluid containing micrococci was introduced into a more dilute solution, there was still an excess of the permanganate, as shown by the color of the solution at the end of two hours. Having determined the germicide power of the permanganate for micrococci, at least for one species of *Micrococcus*, I desired to know whether the oxidizing power of this reagent, when present in excess, would destroy the spores of anthrax, which are recognized as furnishing one of the most difficult tests of germicide power. The following experiments have been recently made:

Nov. 24th, a drop of culture-fluid, containing an abundance of anthrax spores, a pure culture, was added to a considerable quantity of a 4 per cent. (1:250) solution of potassium permanganate. After two hours, two culture-tubes were inoculated with a minute quantity of this material. These tubes were placed in the culture-oven, and the following morning contained an abundance of anthrax bacilli.

Nov. 27th, the above experiment was repeated, except that the time of exposure was extended to four hours. Again there was an abundant development of anthrax bacilli in the culture-tubes, showing that the spores had resisted; but in one tube the development was delayed, and it was only on the morning of the second day that floculi of *bacillus anthracis* commenced to appear.

Dec. 2d, the experiment was repeated, with the exception that the time of exposure was extended to four days. The bacillus now failed entirely to develop in the culture-tubes, showing that the spores had been killed by this long exposure.

¹ Bulletin National Board of Health, July 23, 1881; also, Studies from Biological Laboratory of Johns Hopkins University. Vol. ii., No. 2.

It is probable that, in experiments in which the permanganate is present in excess, the amount present is of less importance than the time of exposure, and that a much stronger solution would fail to destroy anthrax spores in a short time. The wide difference in the potency of this agent and of mercuric chloride is shown by the fact that 1:250 failed to destroy anthrax spores in four hours; while, according to Koch, 1:10,000¹ of corrosive sublimate destroys these spores in ten minutes.

I expect before long to make a special report upon mercuric chloride as a germicide, and in this paper will endeavor to give a summary of the experimental data at present on record, and a detailed account of my own recent experiments with this agent.

Continuing to quote from Bartholow, we find the following:

"We must acknowledge a certain sense of scepticism in examining these experiments of Dr. Sternberg, as able and well-considered as they unquestionably are. We find it difficult to understand why liquor zinci chloridi has no germicide action, whilst a 2 per cent. solution of the same is effective in the proportion of 1 part to 50."

Dr. Bartholow's scepticism is well founded from his point of view, but it results from a misapprehension. I do not say that liquor zinci chloridi has no germicide action, but I record its failure to destroy the micrococcus used as a test-organism in the following experiments: Commencing at 1 per cent.—the time of exposure being, as in all of these experiments, two hours—and meeting with a failure, the following experiments were made, 2 per cent., 4 per cent., 8 per cent. Meeting with a failure in every case, I proceeded to test the pure salt—zinc chloride. This failed in the proportion of 0.5 per cent. and of 1 per cent., but was successful in the proportion of 2 per cent. (Experiment in duplicate, see Table No. I.) In my Table No. II., this result is stated as follows:

"Zinc chloride (2 per cent.) 1 part in 50."

The misapprehension has arisen from the fact that Prof. Bartholow has taken this to mean that 1 part in 50 of a 2 per cent. solution is efficient, whereas it means that 1 part in 50 of the pure salt is efficient, and the 2 per cent. in parenthesis is simply introduced from the previous table as the equivalent of 1 part in 50.

According to *The National Dispensary*, the liquor zinci chloridi should contain about 50 per cent. of the pure salt. If the specimen tested was of this strength, it should have been efficient in the proportion of 4 per cent., or 1:25; but it failed in two experiments in this proportion, and in one in the proportion of 8 per cent. The biological test is sufficiently accurate to make it appear probable that this particular specimen did not come up to the standard as laid down in the *Dispensary*.

¹ In the chapter on Germicides and Antiseptics in my recent work (*Bacteria*), the figures are erroneously given as 1:1000. This mistake resulted from the fact that I had not access to the original report of Dr. Koch, and was obliged to obtain my figures from abstracts in medical journals at hand. Unfortunately, the figures were incorrectly given in the journal from which I

Another sample by the same maker, purchased in a drug store in this city, has recently been tested, at my request, by Dr. Duggan (M.D., Ph.D., and late Fellow in Chemistry in Johns Hopkins University), whose valuable assistance I now have in the prosecution of these experiments. Dr. Duggan reports that this sample failed to destroy the bacteria in broken-down beef-tea (2 hours' exposure) in the proportion of 5 per cent. and of 7 per cent., but was effectual in 10 per cent. It would appear, therefore, that my last experiment (8 per cent. failed) was just within the limits of the proportion which would have been successful if the samples tested were identical in composition.

Dr. Duggan's experiments were made in duplicate, and by the method described in detail in my paper referred to by Prof. Bartholow.

I now pass to the last clause of Prof. Bartholow's reference to my experiments. He says:

"As the experiments of Miquel have been more recently performed, and the conditions insisted on by *Jalan de la Croix* more perfectly observed, we may rather accept his conclusions in regard to the real value of permanganate of potassium as a germicide."

It is, perhaps, open to question whether the experiments of Miquel are more recent than my own. He says, in his work published in 1883:

"Les recherches longues et fastidieuses que j'ai entreprises depuis deux ans sur cette matière me paraissent assez avancées pour qu'on puisse déjà en retirer quelques indications utiles" (p. 291).

My own experiments were published in April, 1883, and there is no evidence that those of Miquel have priority as to date. But this is a matter of comparatively small importance. The question is as to the comparative value of the two series of experiments. As regards the permanganate of potassium, they cannot be compared at all, as my experiments relate solely to the germicide power of this agent upon a single test-organism; while those of Miquel relate to the power of this and other agents to prevent the development of the bacteria of putrefaction in a given quantity of beef-tea (1 litre). Miquel's experiments establish, by a very satisfactory method, the antiseptic power of the agents studied by him, but give no definite evidence as to the germicide power of these agents.

In the case of permanganate of potassium, the immediate effect of adding this agent to a litre of beef-tea would be, if it were added in excess, to destroy by oxidation all of the organic material in the fluid, and to decompose a corresponding amount of the salt. The excess of the salt would then have time to act upon any spores of bacilli and other germs present; but even if not destroyed, these would not develop, for the reason that all of the pabulum suitable for their development would have been quickly destroyed by this powerful oxidizing agent.

In my experiments to determine the minimum quantity of various reagents which would prevent the development of bacterial organisms—that is, to determine the antiseptic value of the reagents named—I purposely excluded the permanganate of potas-

sium, for the reason above indicated. But the experiments with other agents were made by a method quite as reliable as that of Miquel, and, indeed, by one which does not differ materially from it, except as regards the quantity of fluid employed. In these experiments, I added a given quantity of the agent to be tested to a culture-fluid, introduced this culture-fluid into a number of the little flasks which I so constantly use and have repeatedly described, sterilized the fluid in the flasks by long boiling, and then tested the power of the various test-organisms to develop in the flasks in the presence of a given quantity of the reagent under trial. By multiplying these experiments, and adding more or less of the reagent, according as development occurred, or otherwise, I determined the minimum quantity of each reagent which would restrict development. I found, for example, that all of the test-organisms multiplied freely in the presence of 0.1 per cent. (1:1000) of carbolic acid, and that none of these organisms multiplied in the presence of 0.2 per cent. (1:500).

In regard to the method which I have employed and continue to employ, I would say, finally, that it fully meets the most rigid exactions of science; that it has the approval of experts in chemical and in biological investigations at the Johns Hopkins University who are familiar with it; and that I shall be very happy to demonstrate its special advantages to Prof. Bartholow, or to any physician or sanitarian who may desire to satisfy himself of the reliability of the results which I have reported, or may hereafter publish, if he will do me the favor to call upon me at the laboratory where I am now engaged in extended experimental investigations in this direction.

I have considered it necessary to be thus explicit, because it is evident that the misapprehension which has led so distinguished a physician and teacher as Dr. Bartholow to express a certain scepticism in regard to my experiments relating to permanganate of potassium, may also influence others for whose benefit this voluntary work has been done, and thus neutralize the value of my efforts for the advancement of exact knowledge in this direction, and take away the only reward which an investigator can hope for, viz., the appreciation and confidence of his professional confrères.

The experiments of De la Croix, like those of Miquel, have reference especially to the antiseptic power of the agents tested by him. He makes the statement, however, that one part of potassium permanganate in thirty-five, kills the bacteria of broken-down beef-tea. This statement is, no doubt, true, under the conditions of his experiment; but, as I have shown, the result depends upon the time of exposure and the amount of organic matter present, quite as much as upon the proportionate amount of permanganate with reference to the quantity of fluid operated upon. If we add one gramme of permanganate to a litre of broken-down beef-stock, it is quickly decomposed, and no germicide effect is produced; but if we add one drop of putrid beef-tea to a litre of distilled water containing one gramme of permanganate, the organic matter, and the germs as

well, contained in this drop of fluid are quickly destroyed by oxidation.

Several English investigators—Notter,¹ Calvert,² and Tripe³—have attempted to determine the value of potassium permanganate as a “disinfectant;” but the methods employed have not been such as could give satisfactory and definite results, although these earlier experiments demonstrated the value of this agent as an antiseptic and deodorizer.

Other English investigators—Baxter,⁴ Braidwood, and Vacher⁵—have adopted a different test, and their results are interesting and valuable.

These gentlemen operated upon vaccine lymph, and the test of disinfection was the failure of this lymph to produce characteristic vesicles upon the arms of children not previously vaccinated. Comparative experiments were made in each case with lymph not subjected to the action of the disinfectant.

In Baxter's experiments, one part in 200 was successful in destroying the specific virulence of vaccine lymph; and in those of Braidwood and Vacher, a like result was obtained by adding two drops of a solution of 1:120 to “a tube of lymph.”

From what has been said, it is evident that, while potassium permanganate has decided germicide and antiseptic power, it is not generally applicable for purposes of disinfection, because of the readiness with which it is decomposed by organic matter. It is, however, a prompt and valuable deodorizer.

Finally, I have a practical suggestion to make in regard to the use of this agent. We have ample experimental evidence showing the germicide potency of mercuric chloride; but in domestic sanitation there will always be some apprehension of accident on account of the poisonous character of this agent, and the absence of all color or odor in a solution containing it.

Now, the permanganate of potassium has an intense color in dilute solutions, and there would be no danger that a liquid containing it might be mistaken for a potable fluid. No chemical reaction occurs when corrosive sublimate and the permanganate are mixed in the same solution, and I propose the following as a standard “disinfectant” for domestic and general use—for the special purposes for which such a solution would be applicable, viz., the disinfection of sputum (in diphtheria, tuberculosis, etc.), and of liquid feces (in cholera, typhoid fever, etc.):

Bichloride of mercury and permanganate of potassium, of each two grammes. Distilled (or boiled and filtered) water one litre.

This gives exactly one part of each of the salts to 500 of water. A solution of two drachms of each of the salts to a gallon of water would come near enough to this strength for practical purposes.

¹ Dr. J. Lane Notter, Dublin Journal of Medical Sciences, vol. lxxviii. (1879), p. 196.

² Dr. Grace Calvert, Chemical News, London, vol. xxii. (1870), p. 281.

³ Dr. John W. Tripe, Sanitary Record, London, vol. ii. (1881), p. 201.

⁴ Dr. E. B. Baxter, Report on the Experimental Study of Certain Disinfectants. Report Medical Officer Privy Council, etc., N. S. No. vi. (1875) p. 216.

⁵ British Medical Journal, London, vol. ii. (1876).

Experiments recently made indicate that this would be an efficient disinfectant for liquid feces, etc., if used in an amount equal to the material to be disinfected. (Full details of these experiments will be published hereafter.) The permanganate in this standard solution, in addition to the protection which its color would afford, has the advantage of acting as a prompt deodorizer, while the bichloride has no deodorizing power.

It must be remembered in practice that the germicide power of such a compound is not instantaneous, and it will be necessary to leave the material to be disinfected for an hour or more in a vessel with the disinfectant before it can be safely thrown into a privy-vault, or other receptacle where fresh pabulum for pathogenic bacteria may be present.

REMARKS UPON CHRONIC CONTRACTED KIDNEY WITH NORMAL URINE,

INCLUDING ACUTE GOUTY DEMENTIA WITH A PERFORATING RECTO-VAGINAL ULCER, AND DEATH FROM SUDDEN PULMONIC OEDEMA.¹

BY H. C. WOOD, M.D.,

CLINICAL PROFESSOR OF NERVOUS DISEASES IN THE UNIVERSITY OF PENNSYLVANIA, AND NEUROLOGIST TO THE PHILADELPHIA HOSPITAL.

MRS. L., an intelligent lady, about forty years of age, the mother of five children, of gouty ancestry. At regular intervals of four years she was accustomed to have very violent acute attacks of gout or rheumatism associated with great systemic disturbance and depression of spirits. As the last gouty sickness was in the early spring of 1880, she was in the spring of the present year in great fear of another attack, and an attempt was made under the direction of her medical adviser, Dr. Tomlison, to ward off such sickness by careful diet and free horseback exercise. For the early notes of her case I am indebted to Dr. Tomlison. An exposure to cold during the night of April 20 was followed by severe coryza, vague pains, and great hebetude with a very pronounced desire to sleep. Even when moving about she seemed unable to keep her eyes open. Under treatment she improved temporarily. Dr. Tomlison writes concerning this period, "I could discover nothing wrong with the urine, which she passed in usual quantity. A week later she began to have difficulty in expressing herself; she would use irrelevant words and then correct herself; her gait also grew uncertain, and in walking she would pitch forward as if she were going to fall. April 28, Mrs. L.'s mother died; Mrs. L. was greatly shocked, and rapidly became worse; she was greatly depressed, lachrymose, hysterical, had hallucinations, and ceased to recognize those around her. She lost her appetite, and became constipated. The uncertainty of movement now affected the arms, and there was great failure of memory. Her symptoms continually deepened, the speech became more and more incoherent, until it was a confused, senseless jargon. She now refused

food, and finally staid in bed in a state of perpetual stupor. The tongue was heavily coated, the breath very offensive. There was no elevation of temperature, or pains, or local soreness."

I first saw her May 8; I found her in bed in a sort of stupor, out of which she was with difficulty wakened. Getting her partially aroused, I ordered the nurse to put on a wrapper; then commanding and leading the patient, succeeded in getting her to the head of the stairs, then down stairs. It was necessary to hold her very forcibly, as every few minutes her knees would seem to give way and she would "flop" to the floor. All this time she said nothing other than incoherent protestations. When she finally was in the parlor, I upbraided her loudly and severely for her dishabille and general appearance. On asking her if she was not ashamed of herself, she said she was, and that she had better go upstairs and change her clothes. On my acquiescing, she stood up, and taking the hand of her nurse walked upstairs, dressed herself with assistance, and came down, recognizing people, but saying very little. I left her eating her breakfast. Her urine was examined at this time; it was loaded with uric acid and urates, although she had been eating very little, had a specific gravity of 1024, and contained no tube-casts, and not the faintest trace of albumen.

She came to the city May 11. She was now completely demented, knowing no one and not recognizing in any way her surroundings. The pupils were contracted and immobile. She ate no food except milk, which was forcibly given her at regular intervals. Much of the time she lay in a stupor in bed; then she would have spells of wandering restlessness; again distinct maniacal outbreaks accompanied by violence and indecent speech, or sometimes attacks of muttering delirium. The tongue was brown, dry, coated to the last degree. The teeth were loaded with sordes; the breath horribly offensive; the bowels were obstinately constipated. She was treated with purgatives, quinine, chloral, and morphine when excited.

During these days there was general tenderness, so that whenever she was taken hold of roughly she would scream out, even rousing from a stupor. There was also on movement distinct pain, not located in the joints, but seemingly in the muscles. There were very bad hemorrhoids, and as times the patient lay stupid but moaning, with knees drawn up as though there was abdominal pain. The pulse was quick, never much under a hundred, small and feeble rather than strong. Her mental condition grew worse, she took no note of anything, had to be catheterized, etc.

By May 16, the general tenderness had become very pronounced; the pupils were dilated and movable; the intelligence somewhat improved, in that she began to take notice. Salicylates and digitalis were at this time being used freely. On 17th, severe diarrhoea with involuntary passages set in; also the pulse altered suddenly its character, becoming excessively irregular, from 110 to 150 per minute, with beats of all sizes and rates, and many complete intermissions. General tenderness very marked. Auscultation of the heart showed the first sound very weak

¹ Read before the College of Physicians of Philadelphia, Nov. 5, 1884.

over the right base; at the left apex the first sound was very weak, the second decidedly accentuated. Over the middle cardiac region the sounds were singularly confused, with a peculiar watery and occasionally grating sound, "believed to be cardiac friction, but no clear, positive, to-and-fro friction r le." A blister was applied over the head and one over the heart.

May 18, menstruation had been established; the pulse had become perfectly regular, 100, and the peculiar middle cardiac sound less distinct. Her mental condition so far improved that she indicated when she desired to pass water, but she could not give a coherent answer to the simplest question.

On the 20th, she, when roused, answered simple questions with some rationality. Severe diarrhoea again manifested itself with involuntary passages and lasted many days, indeed, off and on, almost to the end; remedies simply kept it in check. There were no maniacal outbreaks, and a slow but progressive improvement in her mental condition began very distinctly directly after the violent diarrhoea of the 20th. The first change was in the recognition of her husband; then, when she had a desire to stool, she would insist on getting out of bed to the commode, although she never said why she got up. Then she resisted food and medicine, clearly because she had a will not to have them. On the morning of the 20th she took food herself, knew where she was, and wondered greatly as to the way in which she had got there, having no memory of past events.

June 23, it was first noticed that something was wrong with the vaginal discharge, but a vaginal examination failed to detect any abnormality. On the morning of June 25 feces were plainly passed per vaginam, and there was discovered a perforation of the recto-vaginal wall very low down, sufficiently large to admit easily the whole forefinger into the rectum. The edges were soft and not well defined. There was no hemorrhage, local swelling, or pain during the formation of this opening, and it was not distinctly sore.

The mental condition of Mrs. L. continued to improve until about the first of June. At the same time her tongue cleaned and her breath became sweet. June 1, she was entirely rational, recognized her surroundings and friends, servants, etc., in their proper relations. She spoke very sensibly about her own illness. The memory was very much improved, but by no means normal, and there was an absolute lack of power of mental exertion; but the only thing a casual observer would have noted as peculiar was the character of her voice, which remained very unnatural.

The first indication of a relapse occurred about the first of June, in a renewed coating of the tongue; then she began to talk irrationally at night, and her memory rapidly to fail. Then a tendency to drop or elide words from her sentences came on and was very pronounced. Her talk in the night began to be irrational and incoherent. At times she was quiet during the day, at other times very restless. At night she was very wakeful and restless, getting out of bed, wandering about rooms, etc. Much of the time she had an almost uncontrollable desire to pick

at her nose and genitals; some days she refused food, others took it. The pulse ranged from 100 to 120, and her physical strength distinctly increased.

From this time onward her mental state deteriorated rather than ameliorated. She lost power of knowing those about her, although she still recognized her husband; the memory was entirely lost; in a word, she was in a condition of almost complete dementia. July 18th, the breathing became suddenly accelerated, and some fine subcrepitan r les were heard posteriorly; on the morning of the 18th, her breathing grew much worse; the whole upper lobe of right lung was full of very fine crepitation, the left lung also containing r les. This continued for two days with some slight dullness of percussion on left apex, and then gradually subsided. Some days after this she had an equally sudden attack of cedema of the feet. June 24, she was taken about 11 P.M. with violent dyspnoea accompanied by fine crepitation anteriorly and superiorly in both lungs, and almost complete absence of breath sounds over the posterior lobes. The dyspnoea steadily increased and she died asphyxiated after twenty-four hours of struggle.

Autopsy.—Kidneys large, plainly in the early stage of chronic interstitial nephritis. Heart somewhat hypertrophied; valves normal. Much excess of fluid in pericardium, but no exuded lymph. Lungs highly cedematous; considerable peritoneal as well as pleural serous effusion.

Brain: Basilar arteries; walls very much thickened, sufficiently so to interfere distinctly with lumen; smaller arteries also showing signs of similar endarteritis; the upper and to some extent the basal sub-membranous spaces everywhere distended with exuded fluid; fine vessels of the membrane somewhat congested; a very little lymph in some spots in the membranes. Brain substance very an emic; the convolutions appeared shrunken; consistence normal, no spots of softening or macroscopic changes to be detected; microscopical examination of the convolutions failed to demonstrate anything abnormal, although the cells were perhaps more granular than normal.

There are certain points about this very remarkable case to which it may be worth while to direct the attention. The cause of the dementia cannot positively be asserted; but I am very strongly inclined to think that it was, at least in part, due to gouty irritation.

It seems to me well established that gout is capable of causing almost every form of insanity; indeed, insanity is only an increase of the mental conditions frequently seen in lith emia. Carrol, in 1859, said, "Gouty mania is occasionally seen," and in 1875, Dr. P. Berthier (*Des Neuroses Diathesiques*, Paris) published a collection of 46 cases of nervous disease attributable to gout: 1, hallucinations; 1, migraine; 4, tetanus; 3, chorea; 1, hypochondria; 7, epilepsy; 1, paralysis, and 26 of mental affection, including in these dementia, melancholia with stupor, mania. Although in some of these cases the evidence is not at all positive that gout was the *materies morbi*, yet in others the relation seems to have been clearly made out.

In his paper before the International Congress of London (vol. iii. 640), Dr. Raynor supported the following conclusions:

1. Protracted gouty toxæmia when not very intense, usually results in sensory hallucinations or melancholia.

2. Sudden and intense toxæmia results in mania or epilepsy.

3. Intense and protracted toxæmia usually results in general paralysis.

4. If there be a tendency to vascular degeneration from plumbism, alcoholism, etc., varying degrees of dementia are produced.

In the discussion which followed the reading of Dr. Raynor's paper, Drs. Savage and Crichton Browne, of London, both expressed the belief that gout does cause insanity, the latter, however, qualifying by the statement—only where there is hereditary predisposition to insanity.

Further proof of the connection between gout and insanity may be found in the Paris Thésis of M. Belliard (1882, No. 269), in which are detailed various cases.

The facts that, in Mrs. L., the attack was at the time when an explosion of gout was to be expected; that in all her previous attacks mental depression was a distinct feature; that her urine was loaded with lithates, although she was taking very little food; and that there was widespread exquisite tenderness and soreness to movement, with febrile reaction, appear to establish a gouty etiology. The contraction of the lumen of the basilar arteries was seemingly sufficient to check the freedom of blood-supply to the brain. Brain anæmia certainly existed, as was proven by the autopsy, and no doubt it aided in causing mental weakness.

It is certainly worthy of remark, as confirmatory of the generalizations made by Dr. Raynor, that the type of mental disturbance exactly corresponded with his conclusions. There was a pronounced tendency to vascular degenerations, and the mental disturbance partook of the nature of dementia.

Passing from the discussion of the etiology of the case, we note the rapid formation of a recto-vaginal circular ulcer as most extraordinary; its occurrence was entirely spontaneous. It seems to resemble in its nervous pathology the rapid eschars which sometimes form upon the buttock and extremities in severe myelitis, or, perhaps, even more closely the so-called perforating ulcer of the foot.

The temperature sheet of this patient was remarkable, owing to the differences between the two axillæ; although, as the autopsy showed, there was no focal brain lesion to account for such difference.

Irregularities of local temperature are well known to occur in diseases of the brain, and the temperature in the two axillæ in the present case was first tested for diagnostic purposes. The result shows that we may have such irregularities of temperature when there is no local lesion. In a case now under my care at the Philadelphia Hospital, believed to be suffering from an acute myelitis, the temperature for many days varied in the two axillæ from 0.2° F. to 1.5° F. To my mind, it is evident that we need careful bilateral studies of temperatures in various diseases.

The point to which I want to direct especial attention, however, is that the urine was examined at various times by Dr. Tomlison without his finding any evidence of contracted kidney, although such lesion existed. My first glance at the patient made me think that she had chronic Bright's disease, but a very careful examination of the chemical reactions, the specific gravity, and the microscopic deposits of the urine so entirely failed to justify any suspicion that I was entirely misled in this feature of the case. I should here state that my own examinations of the renal secretion were so entirely in accord with the statement of Dr. Tomlison, that they were not, as they ought to have been, repeated upon various specimens of the urine. As already stated, the aspect of Mrs. L. suggested the existence of chronically contracted kidney, but there was no increased arterial tension; the heart's action, whilst she was under my care, being uniformly feeble. It is many years since I ceased putting confidence in the absence of albumen as being of much value in disproving the existence of contracted kidney, but I have hitherto believed that reliance could be placed upon the specific gravity of the urine. The importance of examining the specific gravity of the urine cannot be overestimated; and the import of a persistent specific gravity of 1010 or under can scarcely be mistaken; but in addition to the case just detailed, the following is of great interest as indicating that normal urine may accompany a fatally diseased kidney.

Mrs. —, aged 58, the mother of two healthy children, consulted me in the month of April, 1883, on account of certain spells which afflicted her. The history she and her daughter gave was in brief as follows: The attack first began in 1876, about the time she ceased menstruating, and had continued ever since; they always came on when the stomach was empty, and were sure to happen if at any time during the day she was more than three hours without food; also if she does not get breakfast upon rising, she is sure to have a seizure; excessive fatigue increases the tendency to attacks. The spell commences with extreme pallor of face, and dark rings under her eyes: if walking her gait becomes very slow, if talking her speech slackens and then ceases; she looks around in a dazed, bewildered manner, but does not fall, and is not convulsed at all; she does not become completely unconscious, but does not know where she is or what is going on about her; if a mouthful or two of food be forced down her, she arouses immediately, but has no memory of what has occurred during the spell. A careful examination of Mrs. — resulted in complete negations, so far as organic disease was concerned. The urine was normal; there was no failure of mental power, choked disk, palsy, headache, or other local symptom discoverable, and I finally settled down to the diagnosis of gastric vertigo. Under appropriate treatment, the patient improved, and I saw her at my office for the last time July 4, 1883.

In April, 1884, I was hastily summoned to her bedside, and found her comatose, with a history of distinct convulsions, which were said to have been diagnosed as hysterical by a neighboring practitioner, who I was also told, after examination of the

urine, had stated positively there was no disease of the kidneys. On post-mortem examination, the brain was found normal, but the kidneys presented the gross appearances of advanced contracted kidney; and careful microscopical examination by Dr. G. A. Piersoll proved that the condition of the organ was as it appeared.

A second case bearing upon the matter in hand, is that of Mrs. I. W. T., a large, stout woman, who came to my office early last March on account of failing eyesight. Her appearance and description of her symptoms led me to think that she had albuminuric retinitis; but without any examination, I sent her to Dr. Harlan, who reported that she had unmistakable albuminuric retinitis, and that no local treatment would be of service.

Her urine had, the day of her return to me, a specific gravity of 1020, and with the nitric acid gravity test yielded no cloud, or one so faint that I could not be sure that it existed, and noted, "believed to have a trace of albumen." According to her estimate, she was passing two and one-quarter pints a day.

April 3.—She passed three and a half pints, having a specific gravity of 1010, and totally free from albumen.

21st.—Urine three pints, specific gravity 1015; no albumen.

The symptom of increased arterial pressure and cardiac hypertrophy, upon the diagnostic value of which stress has been laid, afforded in the group of cases here narrated no aid. The patients were all large, stout, middle-aged, married women, with full busts, making the recognition of a slight degree of hypertrophy exceedingly difficult, and the circulation in the two more serious cases was certainly enfeebled.

The practical conclusion to be drawn from these cases is, that reliance cannot be placed upon a single examination of the urine, but that in any doubtful case of chronic disease it is our duty to examine the renal secretion repeatedly, noting whether albumen appears after a heavy meal of flesh, and whether the urine of abstinence is of abnormally low specific gravity. I have seen patients who certainly did not have Bright's disease, but in whom an irritant drug or an alcoholic excess would produce albuminuria. It is to my mind very probable that such people will eventually develop renal disease. At any rate, these cases have suggested to me that possibly, as we employ purgatives to make a so-called therapeutic test in a case of suspected typhoid fever, so we might use cantharides, turpentine, or other irritant drug in a case of suspected Bright's disease. If on trial it should be found that a slight irritation would seriously affect the urine, the case should be looked upon with the greatest suspicion.

MEDICAL PROGRESS.

ATROPIA IN CHOLOROFORM INHALATION.—Referring to death by syncope during anæsthesia by chloroform, Poirier refers to the experiments of MM. Dastre and Morat, in which they utilize the known power of

belladonna to paralyze the moderator nerves of the heart. Experiments were first made on dogs, and the results obtained were so encouraging that Aubert, Gayet, and Tripier, surgeons, of Lyons, were induced to try it on the human subject, and they obtained gratifying results.

The following solution was employed:

| | |
|--------------------------------|---------|
| Atropiæ sulph., | 1½ gr. |
| Morphiæ hydrochlor., | 1½ grs. |
| Aquæ dest., | 2½ 3. |

Hypodermic injection of a Pravaz syringe-ful was given from twenty to thirty minutes before the inhalation of the chloroform. The advantages of this procedure, which is now daily practised in the hospitals of Lyons, are the following: Security, greater rapidity with which anæsthesia is produced, absolute quiet of the patient, ease with which consciousness returns, and simplicity of results.—*Le Progrès Médical*, Dec. 13, 1884.

THE INCUBATION OF VARIOLA.—In an interesting paper on this subject in the *Revue de Médecine*, December, 1884, M. VINAY arrives at the following conclusions:

1. The mean duration of the period of incubation is from eleven to twelve days. It appears to be shorter in the hemorrhagic form, in which it is usually but six or eight days. But in the latter, the variations of duration do not bear any relation to the gravity of the disease.

2. Incubation is not prevented, nor its progress modified, by the presence of a preceding febrile disorder. Vaccination alone prevents or modifies the eruption of variola.

3. It is doubtful whether at this period contagion by simple atmospheric media can be caused, but the disease may now be transmitted by inoculations of blood or by vaccination. This last occurrence may result especially during an epidemic of variola. To prevent it, it is preferable to use only animal vaccine, and if it be impossible to obtain this, vaccination from arm to arm should not be practised. It is better to recultivate the vaccine, to keep the vaccinifer under observation, and not use the product within twelve or fifteen days.

4. Not only does vaccination prevent the development of smallpox when it is practised in season, but it also moderates the eruption—lessens it and renders it more benign, even when practised after variolous infection and during the incubation. Success is best assured when it has preceded the appearance of the variolous fever quite a number of days, and is almost *nil* within three or four days. When the fever has appeared, it is useless to resort to vaccination. Whatever the procedure—pricking, scarification, or subcutaneous injection—the progress and severity of the disease are not at all modified.

THE TREATMENT OF OZÆNA.—LOEWENBERG, in an instructive article in *L'Union Médicale*, Dec. 2, 1884, on Ozæna, remarks that the discovery of living microbes in the discharges is an indication for energetic parasiticide treatment. To this end he employs:

1. The *nasal douche* with a solution of 1 part (of corrosive sublimate) to 9000 or 10,000 parts of water, gradually increasing the sublimate, so long as the patient can endure it. That the veil of the palate be

elevated during the application, it is recommended that the vowel *a* be sounded.

2. The *nasal bath*, which is accomplished as follows: The douche being completed, the patient inclines the head backwards, so that the nostrils become the most elevated point of the naso-pharyngeal cavities. While in this position, the solution of corrosive sublimate is gradually introduced through one nostril till it is at the point of escaping by the other, the patient all the time breathing through the mouth, or sounding the letter *a*. It is thus made certain that both nasal fossæ are filled, since the fluid in them conforms to the hydrostatic law of communicating vessels.

3. After the douche and nasal bath, the treatment is terminated each day by insufflations of an *impalpable powder of boric acid*; it is necessary to proceed with great care so as to spread the powder uniformly upon the whole interior of the nasal fossæ and upper pharynx, especially when there is much lateral deviation of the septum. The sounding of the letter *a* is here generally recommended to prevent the acid from reaching the larynx.

The results of the above treatment much excel those obtained by the nasal tampon, which is objectionable on account of retaining fetid discharges. The method as indicated above, destroys the fetidity of the discharges, and marked improvement of the general health follows its use.

TREATMENT OF SYPHILIS.—At the International Medical Congress of Copenhagen the treatment of syphilis by hypodermic injections was ably discussed by various members. PROF. LEIBREICH considered formiamide of mercury the best preparation for hypodermic use, since it is extremely soluble and very slightly irritating.

MARTINEAU asserted that by this means the physician is certain to cure all manifestations of syphilis—normal or abnormal. The form of mercury preferred, according to his experience, is the peptonate of mercury and ammonia.

WOLFF (of Strasbourg) used neither the peptonates nor the formiamides, but preferred the following:

1. Carbonate of soda, 23 grains; water, $3\frac{1}{2}$ ounces.
2. Corrosive sublimate, dissolved, 57 grains; water $3\frac{1}{2}$ ounces.
3. Glycocoll, 38 grains; water $3\frac{1}{2}$ ounces.

The glycocoll may be replaced by asparagin or alanin, or, better still, by sarcosin.

Each of the three above preparations is made in a separate bottle, and they are mixed in equal parts at the time of using. Each syringeful of fifteen grains contains exactly one-sixth of a grain of the oxide of mercury. The injection is made two days in succession, the patient being then left one day without treatment. The operation may be repeated twenty-five times without accident.—*Annales de Derm. et de Syphilis*, Nov. 1884.

DISGUISED FORMS OF MALARIAL POISONING.—In an interesting paper with the above title, DR. F. SIMON, in the *Rivista Clinica de Bologna*, shows the necessity, especially in children, in obscure and serious cases, of holding in mind the possibility of such conditions, and insists upon the efficacy of sulphate of quinine in their treatment after all other remedies have failed. He cites three cases in which the symptoms noticeable

were, in one, suffocation, followed by persistent dyspnoea; in another, an intermittent fever and torticollis, manifested twice daily and at regular intervals; in a third, a simple intestinal catarrh, followed by a very profuse, muco-sanguinolent diarrhoea. In every case the administration of quinine was followed by permanent improvement.—*Gas. Med. di Torino*, Dec. 15, 1884.

THOMSEN'S DISEASE AND PSEUDO-HYPERTROPHIC PARALYSIS.—In the *Archives de Neurologie* of November, 1883, DR. ROMAN VIGOUROUX reaches the following conclusions concerning this disease:

1. The hysterical attacks primarily resemble the advent of common epilepsy.

2. Some cases exceptionally simulate an attack of *le petit mal*.

3. Again the attack resembles partial epilepsy (Jacksonian epilepsy, or epilepsy due to cerebral lesion).

4. False partial epilepsy of a hysterical nature is like partial true epilepsy, characterized either by one-sided spasm with rotation of the body and eyes of the convulsed side, or by monoplegia.

5. Partial epileptoid seizures often constitute the whole attack of hysteria, and are followed neither by marked spasm nor by hallucinations.

6. The approach of false partial epilepsy is shown commonly by a series of attacks followed by a true epileptic condition.

7. The symptoms which enable false partial epilepsy to be diagnosticated from true partial epilepsy are:

- a. Absence of pyrexia in false partial epilepsy.
- b. Absence of paralysis of the convulsed limbs in false Jacksonian epilepsy.

c. Less significant, but pointing to the hysterical nature of the case, are the opening of the mouth during the approach of the attack, contractions of the eyebrows, and undulatory movements of the belly at the beginning and during the intervals between the crises.

8. Other useful elements of diagnosis are the effects of ovarian compression, failure of potassium bromide, and establishment of permanent symptoms of hysteria after the attack.

ACTION OF HYDROCHLORATE OF COCAINE UPON THE MUCOUS MEMBRANE OF THE PALATE, THROAT, AND LARYNX.—DR. ARTHUR GEIER, in the *Berliner klin. Wochenschrift* of Dec. 15, 1884, in a paper with the above title, calls attention to the following points:

1. The action of cocaine begins in from one to two minutes after its application and lasts about ten or twenty minutes.

2. Before the objective action of cocaine appears, the subjective results are manifested and outlast for a short time the objective.

3. With the remission of the subjective symptoms the characteristic appearance of the mucous membrane returns as it existed before the application of the cocaine.

4. Anæsthesia need not be complete—i. e., touch may be felt even in very sensitive individuals without reflex action being excited.

5. The subjective symptoms exist in the feeling of numbness, dryness, swelling, and in failure to perceive mucus and saliva in the movements of deglutition.

6. The sense of taste is blunted and the sense of

touch abolished, while the sense of temperature remains.

In all the cases the statement was made that the button of the laryngeal sound felt "cold."

COCA AND COCAINE.—Coca is the leaf of the erythroxyton coca, of the family Malpigiaceæ, and is a native of Peru. The leaves are the only part of the plant used. The use of the coca leaves is very extensive in Peru, Mexico, and Bolivia. Cocaine is an alkaloidal extract of coca; it was discovered by Niemann in 1860, and carefully studied by Lassen, in 1864, who found a second alkaloid, *hygrine*. Cocaine is solid and crystallizes in prisms of from 4 to 6 sides. It is colorless and odorless; soluble in water and alcohol, and is very soluble in ether. It has an alkaline reaction, and saturates acids to form very soluble salts which, with the exception of the hydrochlorate, crystallize with difficulty.

Ecgonine. If, instead of saturating hydrochloric acid with cocaine, it and the acid be heated to 212° Fahr., in sealed tubes, a new base is obtained, to which Wöhler gave the name ecgonine, benzoic acid and methylic alcohol being formed at the same time—ecgonine is very soluble in water, and insoluble in ether. It has not yet been used in therapeutics.

Niemann gives the following as the process for extracting cocaine: The leaves are digested in 85° alcohol, to which a little sulphuric acid is added. Into this is put a slight excess of milk of lime, and the mixture left standing for a time. The alcoholic liquid is then poured off, neutralized with sulphuric acid, and evaporated to the consistency of an extract. A blackish mass is thus obtained which yields its sulphate of cocaine to water. By precipitating this solution with carbonate of soda, cocaine is obtained, which is purified by being dissolved in ether and then crystallized in alcohol.

Lassen prefers the following method: The leaves are exhausted with cold or slightly warm water; the macerated residue is precipitated by the addition of subacetate of lead, the excess of which is then cautiously removed with carbonate of soda. After filtering, the watery liquid, the reaction of which should be only slightly alkaline, is agitated with ether, which dissolves the cocaine. On evaporating this, ether leaves the crude cocaine, or it is purified by dissolving it in water acidulated by hydrochloric acid, and then dialyzing. The hydrochlorate of cocaine passes rapidly through the septum, and is precipitated with carbonate of soda. One kilogramme of leaves yields about two grammes of cocaine.

Hygrine is another alkaloid found by Lassen in coca leaves. It may be obtained from the watery fluid from which cocaine is separated by shaking up with ether. To this fluid an excess of carbonate of soda is added; the lime salts are precipitated and hygrine liberated. It is then obtained by shaking up with ether, and a brown, slightly alkaline liquid is obtained by evaporation. Hygrine is a liquid and volatile alkaloid, with a strong odor of trimethylamine, and forms crystallizable salts with acids.—*Progrès Méd.*, Dec. 13, 1884.

THALLIN.—The industry of continental chemists in the application of the synthetical methods to the formation of new compounds has brought to light a new antipyretic agent, which appears to have been used success-

fully in a large number of cases in the clinic of Prof. Nothnagel. It has the constitution of a secondary chinoline base, being one of a number of chinoline derivatives prepared by Prof. Skraup, and is represented by the systemic name, "tetrahydroparachinanisol." The salts of this base, which have an acid reaction, are freely soluble in water, and have the property of forming green compounds when treated with solution of ferric chloride and oxidizing agents. On account of this peculiarity, the cumbersome systemic name has been dropped in favor of the shorter designation, "thallin." The hydrochlorate, sulphate, and tartrate of thallin and the hydrochlorate of ethyl thallin are the salts that have been employed, and these are said to show great antipyretic activity in doses of quarter, half, and three-quarters of a gramme, a point in which thallin would seem to compare favorably with some of its competitors. The lowering of the temperature is said to take place gradually and to last a considerable time, whilst it is not accompanied by any secondary disturbances. Thallin can be conveniently administered in wafers containing a quarter or half of a gramme of the sulphate. Two other chinoline derivatives have been experimented with; one of them has but slight antipyretic properties, while the other was fatal to guinea-pigs in doses of from 2 to 6 grammes, violent tetanic seizures being excited.—*Lancet*, Dec. 6, 1884.

COCAINE AS A URETHRAL ANÆSTHETIC.—DR. A. BLUMENFELD, in a preliminary note on this subject, reports having used cocaine for producing anæsthesia of the male and female urethra. He first used it in cases in which catheterism was painful, injecting \mathfrak{M} xxx- \mathfrak{xlv} of a two per cent. solution into the urethra by means of a small syringe, and the fluid retained by pressure with the fingers for three or four minutes. In every case the injection produced a diminution of sensibility, and often complete anæsthesia of the whole urethra, as far back as the neck of the bladder. The anæsthesia persists for half an hour, and may be kept up longer by a second injection or by a stronger solution.—*Deutsche med. Wochenschrift*, December 11, 1884.

PHYSIOLOGICAL ACTION OF BROMOFORM.—VON HOROCH reports that his experiments on animals with bromoform, administered subcutaneously, by the stomach, or by inhalation, show that this drug is a general anæsthetic. The respiration is not notably influenced; the pulse continues strong and of normal frequency; and the peripheral ends of the vagus retain their irritability. The blood-pressure falls during narcosis, but rises again with the discontinuance of it. The irritability of the vaso-motor centre is lessened, and the reflex irritability is entirely lost.

Administered hypodermatically, a comparatively small dose causes deep and lasting narcosis; due, not so much to the size of the dose, as to the ease with which the drug is absorbed. Von Horoch has had no case of abscess at the seat of injection, nor any noticeable irritation from it. He has not given it to the human subject by inhalation, but it does not cause vomiting in animals. The vapors cause considerable irritation of the conjunctiva.—*Centralbl. f. d. med. Wissensch.*, Oct. 25, 1884.

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SATURDAY, JANUARY 10, 1885.

PEPTONURIA IN THE PUERPERAL STATE.

WHILE the true significance of peptonuria is as yet undetermined, there is no doubt that it plays a part in a large number of processes, the majority of which are pathological. Recently DR. WILLIAM FISCHER has published, in the *Centralblatt für Gynäkologie*, Nov. 11, 1884, some investigations on the occurrence of peptonuria in puerperal women, and has found that peptone is present in the urine in a large percentage of cases. Thus, in 156 tests of urine from 56 lying-in women, he found peptonuria 86 times. During labor and the twelve hours immediately succeeding it, he found no peptone. During the second and third day peptonuria is almost constantly present, having been found in 24 out of 25 examinations. From the fourth to the sixth day it was found in 37 out of 44 examinations; from the seventh to the tenth day in half the specimens examined. After that time there were two doubtful reactions, but peptonuria could not be asserted.

Eliminating all but the most typically physiological cases, there still remain 15 primiparæ and 25 multiparæ in whom peptonuria was found in about the above proportion. It occurred with living and still-born, full-term and premature births, and disappeared more rapidly the more rapid the involution. In contrast to this the urine in a case of Porro's operation, examined for six days after the operation contained no peptone.

In seeking for the source of such a peptonuria it is to be remembered that Hofmeister has shown that it attends inflammatory and suppurative processes, and that, therefore, suppuration about the external or internal genitalia may be responsible for it in puer-

peral women. But Fischer ascertained that in his cases the external genitalia, at least, might be excluded with certainty, as these furnished in the primiparæ the most trifling, and in multiparæ absolutely no abrasions, and still less suppuration. Fischer himself learned that the lochia frequently contain peptone, but ascertained that during the second and third day it was seldom present in them, while the urine examined at this time always contained it. That the leucocytes in the puerperal uterus were also not responsible for the peptonuria seems likely, from the fact that their number is immensely less than in an inflamed lung, accompanying which the peptonuria is much less marked than that in the puerperal state.

Fischer is forced, therefore, to the conclusion that under certain circumstances the fatty degeneration of the muscular fibres, which forms a part of the process of involution of the uterus, is attended with the conversion of muscle albumen into peptone. Certainly in the substance of the fresh puerperal uterus, and almost exclusively in the muscular element, peptones are found. This Fischer has confirmed by the study of fresh specimens derived partly from operations—the hyperplastic uterus of tumors—and partly from rabbits killed sixteen and forty hours after delivery. On the other hand, he found no peptones in the non-pregnant uteri of animals, nor in pregnant uteri which had not undergone contraction. In a single instance only did he find peptone in the endometrium.

With a view to comparing this condition with analogous ones, Fischer produced experimentally phosphorus poisoning, thinking that the fatty degeneration induced by it in the albuminous tissues would afford an analogy to the fatty degeneration of the puerperal uterus and result also, perhaps, in an analogous peptone formation. In nine experiments he found peptone in several organs, of which two were tissues which, in the normal animal, never contain peptone, viz., the liver and striated muscular tissue. It was found in organs which had not as yet undergone fatty degeneration. Fischer also examined the urine of pregnant women for peptone, and found it in one-fourth of the sixty-eight investigated. With regard to its cause under these circumstances, he only ventures to suggest that it cannot have anything to do with internal suppuration, and that it may be due to contractions of the muscular structure of the pregnant uterus.

DR. OTTO KÜSTNER reports, in *Centralblatt für Gynäkologie*, November 22d, a case of peptonuria incident to rupture of an ovarian tumor into the abdominal cavity. The patient came under observation July 21st, and the next day, during an examination of the case while under ether, the sac was burst.

The urine of that and the next day, amounting to but 300 grammes, contained only a trace of peptone; on the third day, however, after the rupture, the urine gave a decided reaction with phosphotungstic acid.

The explanation furnished is that there are always peptones in the contents of such cysts; but there is no absorption of the peptones from the contents of the unruptured cyst, because of the poverty of blood-vessels in its walls. But should a cyst rupture into the abdominal cavity, the peptones are immediately absorbed, and secreted by the kidneys, peptonuria being the result, as soon as the quantity of peptones in the blood reaches a certain amount.

The intensity of the peptone reaction is always proportionate to the rapidity of the absorption of the peptone-charged fluid. The diagnostic application of this observation is found in the fact that, after the rupture of the cyst, percussion revealed practically no diminution in the area of dulness. Hence, in case of ovarian cyst, it may be inferred that the sac has ruptured, if peptonuria suddenly appear.

SUPERFETATION.

In resuming the consideration of that variety of superimpregnation known as superfetation, it may be remarked that ancient authorities, among whom may be mentioned Hippocrates and Aristotle, fully believed in the doctrine. Daremberg has stated that Aristotle derived from Homer his belief in the viability of the fœtus at seven months, an example of the birth of a living child at that time having been given by the Greek poet, in the nineteenth book of the *Iliad*, in which Juno is asserted to have induced premature labor in Sthenelus's wife "scarce seven moons gone." It seems quite as probable that the father of medicine found in one of the versions of the story of Antiope ground for his faith in superfetation. This beautiful maiden becoming pregnant by Jupiter, was driven from her father's house, and kindly received by Epopeus; she gave birth to male twins, one of them, Amphion, who was said to be the son of Jupiter, while the other, Zethus, was the son of Epopeus; certainly the two differed so greatly in character that one might readily believe they had different fathers.

It is more probable, however, that Hippocrates and other ancient authorities believed the doctrine of superfetation from their ignorance of the true form of the womb in the human female, regarding it double as they had seen it in so many of the inferior animals. Now it so happens that in some of the cases in which superfetation has been alleged, duplicity of the uterus was proved to be present, arrest of development having occurred, so that this organ retained a temporary form in the human female, but the permanent form of inferior animals. It certainly

would seem to be much more probable that superfetation would occur in a case in which the womb was double than where it is single; since in such a case the mechanical obstacle to the entrance of the spermatozooids and their ascension in the uterus which, after a certain period of pregnancy presents an impassable barrier, is not present, for of course the second half of the uterine cavity has not undergone the changes consequent upon pregnancy. It must be obvious that if superfetation were possible, or at least if there were not very great obstacles to it, multiple pregnancy would become the rule rather than the exception, since woman is less favored than animals, who are exempt from sexual intercourse during pregnancy.

The presence of the cervical plug of mucus, which was supposed to be formed just after the beginning and remain until the end of pregnancy, may be rejected as an inconstant phenomenon, or, if present, furnishing no serious obstacle to the ascension of spermatozooids.

Union between the decidua of the ovum and that of the uterus—the decidua reflexa and the decidua vera, according to the Hunterian theory—does present an insuperable barrier to the passage of spermatozooids. According to Robin and most authorities this union occurs in the fourth month, while Schröder gives the sixth month. It is thus seen that there is no anatomical obstacle to superfetation prior to the fourth month at least.

The strongest argument against superfetation is that which rests upon the suspension of ovulation during pregnancy. As the hen when hatching does not lay eggs, so the human female when carrying on a process of internal incubation does not ovulate. Almost all physiologists are agreed upon this point. Playfair, indeed, asserts that menstruation in pregnancy proves ovulation; but Doléris justly suggests that menstruation after double ovariectomy would then prove the same thing. With Stoltz we believe that no true menstruation occurs in pregnancy, and that all discharges of blood from the uterus in such condition, even if observed periodically, are simply threatenings of abortion, or of premature labor, and should be so treated. Matthews Duncan claims that there is no anatomical reason, during the first three months of pregnancy, why the ordinary menstrual flow should not proceed from the body of the womb, and Panum (*Nord. med. Ark.*, 1882) believes that ovulation does not cease until two or three months after the pregnancy begins. It may be answered that when a woman becomes pregnant nature's vow has been fulfilled, and therefore there is no necessity for the maturing and discharge of other ovules, since single not multiple pregnancy is the law of the human race. Nature does nothing in vain was the axiom of Aristotle, but surely it would be in vain in

almost all cases for ovulation to continue during pregnancy, or any part of it. Schroeder asserts that there is not a single well proved example of the expulsion of an ovule during pregnancy; the same statement is made by Doleris.

We may, therefore, conclude from this brief review that while superfœtation is not impossible, it is in the highest degree improbable. The cases that have been adduced in its favor are either too remote from our times, or too imperfectly reported, or admit of other explanation: some of them seem in part fictions rather than facts. Time fails us to analyze the different cases on record, and show the correctness of our statement.

Most of the cases of multiple pregnancy are doubtless the result not of superfœtation, not, indeed, of superfecundation, but of simultaneous fecundation. It is probable that at each seminal ejaculation a large enough number of spermatozooids is discharged to fecundate hundreds of ovules; nature makes ample provision, probably no less in the male than in the female element, to secure the continuance of the race.

Referring now for a moment, in conclusion, to Dr. Wagner's case of quadruplets, referred to in the last number of *THE NEWS*, was one of the quadruplets conceived a month before the others, or, in other words, was this a case of superfœtation? The arguments in favor of this hypothesis are the greater size, weight, and vitality of one of the children—in a word, its better development. We answer—first, it is rare that even in twin pregnancy the children are equal in size and weight. A similar fact is observed in those domestic animals which produce many at a birth; thus it is common in a litter of pigs especially, to find one very much smaller and feebler than any of its brothers and sisters, and it is commonly called a runt. But no one in such cases attributes inferiority of size to superfecundation, still less to superfœtation.

Again, the difference in favor of the largest of the four children was half a pound in weight and one inch in length. Now, supposing the pregnancy to have begun in November, it ought to have ended about the 12th of August; but it ended the 22d of July, some three weeks sooner than it should have done, and as is well known even in cases of twins, still more in quadruplets, premature labor is the rule. Admitting, for argument sake, that the largest child was conceived in November, the other three in December, and that the former was born at eight, the others at seven months, did half a pound in weight and one inch in length correspond with one month of development? It is fair to state that the increase in the supposed time ought to have been from one to two inches in length and about two pounds in weight.

While, then, possibly superfœtation might have occurred in this case, we must regard it as very doubtful.

A STATE BOARD OF HEALTH AT LAST!

We are glad to learn that a renewed effort is now being made in Pennsylvania to establish a State Board of Health, and so wipe out the disgrace which attaches to this State from the fact that whilst twenty-seven States have organized protection for the health of their people, Pennsylvania and Ohio are as yet unprovided with active boards of health, and are the only important Commonwealths in the Union so destitute. Moreover, if the proposed law for a National Board of Health be enacted, this State will suffer under the disadvantage of not being entitled to representation in that organization. A conference of delegates from various local organizations has been called to meet in Harrisburg on the 13th of January, when it is to be hoped that the powerful arguments in favor of creating a State Board of Health will be so cogently presented before the Legislature as to secure the passage of the requisite Act, and a grant of the paltry appropriation needful to vitalize its provisions. The appeal is more worthy of attention and immediate action at the present time than hitherto, in view of the prospect that cholera, following its usual course in former visitations, will make its appearance upon our shores early next summer. Then, unless active and systematic measures to prevent its spread are enforced over the whole State, it will inevitably entail upon us a vast amount of suffering and death, to say nothing of the pecuniary losses which must infallibly result from its epidemic prevalence.

The outbreak of smallpox in 1872-73 is estimated, by a highly competent observer, to have cost the city of Philadelphia, directly and indirectly, several millions of dollars, and the mere rumor of a probable extension of epidemic cholera unchecked, in Pennsylvania, would involve a loss of trade from fleeing citizens, as took place in Marseilles and Toulon last year, which might readily amount to a hundred or perhaps a thousand-fold the sum required for maintaining an efficient State Board of Health. In 1879 the report of a single case of yellow fever in the South caused a shrinkage of values in the Chicago provision market amounting to one million of dollars in twenty-four hours.

One of the most conclusive arguments against the cavillers, who dispute the efficacy of systematized sanitary precautions in controlling the spread of disease, is to be found in the experience of New York during the visitation of cholera in 1866. According to the official report, upwards of a thousand cases of cholera then occurred in numerous localities more or less widely separated; and in order to prevent the spread of the malady, a well-appointed disinfecting

corps was organized, and arrangements made for squads to respond instantly to any call, either by day or night. They were provided with every requisite, and with means of transportation to secure immediate service; and the beneficial effect of their labors is shown by the statement, that in three hundred and sixty-two New York houses, where individual families or persons were smitten by cholera, but which were promptly brought under sanitary control by disinfection and local purification, the pestilence did not extend beyond the family in which the first case occurred.

CHOLECYSTENTEROSTOMY.

FROM a series of experiments conducted upon dogs in August, 1884, DR. GASTON, of Atlanta, demonstrated the feasibility of establishing a communication between the gall-bladder and the duodenum—an operation which he suggests should be performed upon the human subject in preference to cholecystotomy in cases of closure of the ductus communis choledochus. The paper, which is a most interesting contribution to our knowledge of obstruction of the duct, may be found in *Gaillard's Medical Journal* for October, 1884.

In his comments upon Dr. Gaston's proposition to divert the bile directly into the duodenum, its editor states that the "great, novel, and original operation" should be known as that of Gaston. If he will, however, turn to the *Prager med. Wochenschrift*, Nos. 21 and 22, 1882, he will find that, in a case of closure of the choledoch duct of a woman thirty-four years of age, Von Winiwarer successfully performed the operation which Gaston practised in dogs. Hence any "honor and distinction" which the ingenious and rational procedure may merit, must be awarded to Von Winiwarer, and not to Gaston.

THE BACILLI OF THE MOUTH.

No more fertile source for bacteria, available to all who care to study them, can be found than the cavity of the mouth. W. D. MILLER states, in the *Deutsche med. Wochenschrift* for November 27th, that if a sharp instrument be introduced between the gum and a tooth, especially if the former be from any cause slightly hyperæmic, the scrapings thus obtained leave nothing to be desired in the way of curved bacilli and spirochætae.

In the first place, there is a comma-bacillus resembling that of cholera, but which Koch has shown to be altogether distinct. No efforts to cultivate it have as yet succeeded. A second fungus, less difficult to cultivate, is a short, plump, somewhat pointed bacterium, of which two are often united at an acute angle. The latter especially occur with rods which are in a state of fission. The fungus is mobile, growing very rapidly at the temperature of the room,

and liquefies gelatine to a considerable extent. A third fungus is also a delicate rod, either straight or more or less curved, sometimes to the extent of a semicircle, so that two placed appropriately produce an O-shaped figure, and sometimes the rodlets are so rolled together that they can scarcely be distinguished from coccospheres. By fission they form coccobacilli, best seen in old cultures. In these Miller has found neither spore formation nor motion. They are very slow and difficult of culture on gelatine, but do not cause a liquefaction of this substance. Hence, it differs in this respect from the cholera fungus.

Miller has also found, in the mouth, fungi which are not destroyed by an artificial gastric juice, and these may pass through the stomach into the intestine, and there undergo further development. This may be true of these curved bacilli described, since he has found in his own feces undoubted curved bacilli, especially accompanying a slight diarrhoea. But these were as difficult of culture as the curved bacilli found in the mouth. Whence we may infer that the fact of a bacillus being curved is no criterion of its specificity, and that bacteria of this shape occur which have no closer relation to each other than the different bacteria which are found in the shape of cocci. Much more accurate information may, however, be expected from carefully conducted culture experiments.

DRAINAGE OF CEREBRAL ABSCESS.

IN THE MEDICAL NEWS for August 2, 1884, we called attention to the subject of drainage of abscess of the brain, and stated that up to that time only three cases had been recorded, all of which recovered, the operators having been Burchard, Fluhrer, and Fenger and Lee. In looking over the *American Journal of the Medical Sciences*, we find in the number for July, 1882, that in a remarkable case of lodgement of the breechpin of a gun in the cranium, Dr. Noyes made free provision for the escape of pus from an abscess which subsequently formed in the frontal lobe of the brain, a drainage-tube having been passed by the trephine hole through the cavity and out of the orbit. Death, however, ensued from basal encephalitis.

Unless we greatly mistake, Dr. Noyes led the way on this side of the Atlantic in draining a cerebral abscess; but the first operation of the kind must be credited to Burchard, whose case is published in the *Deutsche Zeitschrift für Chirurgie*, Bd. xv., 1881.

In this connection, we may call attention to the successful extraction of a breechpin from the brain by DR. KEMPER, which is recorded in the *American Journal of the Medical Sciences* for January, 1885. In this case, drainage was also resorted to, thereby showing the great superiority of the new over the old plan of managing such lesions.

SOCIETY PROCEEDINGS.

PHILADELPHIA ACADEMY OF SURGERY.

Stated Meeting, December 1, 1884.

THE VICE-PRESIDENT, S. W. GROSS, M.D.,
IN THE CHAIR.

DR. W. W. KEEN read a paper on

COMBINED TUBULAR AND CAPILLARY DRAINAGE OF LARGE WOUNDS.

The great practical importance of the subject of drainage, he said, leads me to make a few remarks this evening in relation to its use in large wounds.

The *methods* may be divided into two—first by tubes, and secondly by capillary action; and the *materials* vary in each. I reject the older mode by tents and their equivalents, as with modern antiseptic methods they are practically abandoned. For modern surgery demands that we shall strive for union by first intention and without suppuration—results unattainable by the older means just named.

The tubes employed are either rubber (usually fenestrated) or of decalcified bone. They have one marked advantage, viz., their large calibre and free discharge. Hence they are especially fitted for securing the escape of the more abundant oozing following all large operations or after opening large abscesses. I always keep several feet of such tubing in a bottle of carbolized water (five per cent.), and have not found any septic results from their use. They have, however, the disadvantage that if exposed to pressure, as in a joint, their calibre is obliterated. The decalcified bone sometimes is not aseptic and not seldom softens and collapses, especially if kept in carbolized oil. Keeping them in glycerine and alcohol or in the bichloride solution and alcohol often prevents this. But even then, within a week, they dissolve and no longer act as drains.

The second method is by capillary action. For this purpose either horsehair or catgut or rubber threads are used. They answer well in case the liquid to be drained is thin, but if thick and glairy they fail to act and are useless. Of the three, on the whole, I prefer horsehair (which, after washing, I always keep in carbolized water as above), as it is equally unirritating, can always be had, is strong, is not absorbed as the catgut is, and allows the granulations to grow in between its threads.

My special object, however, is to bring before the Academy a method I have used for some seven or eight years which unites the advantages and avoids the dangers of both methods. The chief advantage of the tubing is its free discharge. Its disadvantage, in addition to that noted above, is that if used for any length of time, when removed, it leaves a tubular passage of considerable calibre lined with granulations. This passage if long, as in large wounds, is often apt to close at two or more points in its course, thus penning up the slight discharge and producing retention and suppuration. The disadvantage of the capillary drainage is that it is not fitted to give exit to large amounts of fluid. Its advantage is that it leaves no such tubular passage, but that while giving us the means of introducing tubing for freer drainage, if at any time it is needed, it allows nearly complete healing, even while a few of its strands

are still *in situ*. Especially is this of value in larger wounds with long drainage paths.

The method referred to is as follows: When the wound is ready to be closed, a fenestrated rubber drainage-tube and a bundle of horsehair of fifteen to thirty or more strands are both placed side by side in the wound. At the end of twenty-four or forty-eight hours the abundant oozing of bloody serum usually necessitates a redressing, but by this time the first abundant discharge has ceased. Accordingly at the first dressing after the operation I remove the rubber tube, leaving the horsehair in place. If the oozing will probably be small, I often even remove a large part of the horsehair. At the second dressing, say in three to six days, I remove all the horsehair or all but two or three strands. In doing so I always remove the hairs one or two at a time, as the nice adjustments of the surfaces is thus scarcely at all disturbed. At the third dressing, if all has gone well, the last horsehairs are removed and the capillary passage heals within twenty-four hours.

For joints or in other wounds where possible longer slight discharge may take place, the horsehair may be left for longer periods as judgment dictates.

I have used this method in amputation of the breast, often bringing tubing and horsehair out through a button-hole counter-opening in the axilla and treating it as described above. I have used it in a large number of amputations of the upper and lower extremities and in the removal of tumors of the neck and other parts of the body and find it to work admirably.

The same result may be attained by first using the tubing alone, and replacing it at the first redressing by the horsehair, but the pain and the mechanical disturbance of the wound are so much greater than the method above described that I have never found it to answer as well.

Of course for small wounds, only the horsehair is required.

While speaking especially of rubber tubing and horsehair, this method of combined drainage will answer equally well with any of the other materials mentioned, and it is to the method that I particularly design to call attention rather than to the material used.

DR. J. EWING MEARS said that the objection to the rubber tube is, as has been stated, that it leaves a canal lined with lymph, which may close at different points, preventing the escape of fluid. There is one method which had not been referred to. It consists in introducing a decalcified bone drainage-tube, within which has been placed a number of horsehairs. The hairs are removed one by one. This prevents the collapse of the tube, but does not overcome the objection to the presence of the tube in the wound.

He had frequently practised Dr. Keen's method in the St. Mary's Hospital, where it is in general use, and he knows that it works admirably in the deep wounds met with in hospital practice.

There are, of course, certain objections to the presence of the tubes in wounds, in that they interfere with the reparative processes and prevent union by the first intention. At the same time, however, the advantages gained can scarcely be counterbalanced.

DR. J. M. BARTON said that he had been in the habit of using, in the case of wounds where there are two openings, a rubber drainage-tube, inside of which is

placed a number of rubber threads. When ready to remove the larger tube, the small threads are held securely and the tube drawn off. When there is no counter-opening, it is always difficult to retain the tube in position, and when the tube is removed it is difficult to replace it. In order to facilitate the introduction of the tube under such circumstances, he had recently had the ordinary probe of the pocket-case modified. The sharp, triangular end of the probe has been blunted and a little spur placed at a short distance from its extremity. When inserted into one of the openings of the tube, this spur prevents its passage into the tube.

DR. O. H. ALLIS asked Dr. Keen whether he had ever used fine silk in place of horsehair?

DR. KEEN said that he had never used the silk, although of course it would, in many respects, answer as well as the horsehair. He thought that when a number of threads are made into a strand by twisting, as in the case of a silk thread, the capillary action is not as good as in the horsehair, and the strands are apt to mat together if the discharge be thick. If used at all, it should of course be kept in an antiseptic solution.

In the case of wounds with a single opening, he takes the requisite number of hairs and doubles them in the shape of a hairpin. After the loop is introduced through the external opening, the hairs have enough stiffness to permit of their being readily pushed to the bottom of the wound. Of course, in attempting to remove a portion of the mesh, all of it may be pulled out, but in such a case it is a simple matter to introduce a smaller mesh.

DR. MEARS asked if Dr. Keen had had any experience in the use of catgut threads in the drainage of deep wounds?

DR. KEEN replied that he had, but the hair is so much superior that he had used it almost exclusively. Catgut swells, and it may obstruct, instead of facilitating, the escape of the discharge. Horsehair does not undergo such a change, and is much more permanent.

DR. DE FORREST WILLARD had found the same objections to catgut. It also seems to hold the pus, and not carry it out. He thought that we are apt to leave the rubber tubes in too long, so that they act as setons. If the tube is removed gradually, healing may not occur as rapidly as the tube is removed, and accumulation of pus may take place behind it. In order to have free drainage after amputations, he inserts between each stitch a short piece of rubber tubing. These are removed at the first dressing. There is a caution to be given in regard to this plan in cases in which there are redundant skin flaps, and that is that there is danger of one of the tubes slipping out of sight.

DR. O. H. ALLIS remarked, in regard to capillary action, that a silk thread must have a more powerful action than horsehair; which could be proved by inserting a silk thread and a horsehair into a tumbler of water. The silk would take up the water more quickly than the horsehair. Two hairs, placed side by side, would have greater action than one, and three braided together would have still more powerful effect. I have had good reason to continue the use of single threads passed through the wound in different directions.

DR. MEARS asked the question, in regard to the use of catgut, because it has been claimed that great advantages have been derived from the use of tubes

which are absorbable, such as the decalcified bone drainage-tubes. He thought, however, that the judgment of many surgeons is adverse to their use, because they often undergo absorption too rapidly. It is said that immersion in alcohol renders them more resisting.

DR. S. W. GROSS thought that Dr. Keen's remarks in regard to the use of horsehair are capable of a wide range of application. He had used decalcified bone, and he had been more disappointed with it than with anything else that he has tried in surgery. They absorb the fluids, become soft, and collapse. If soaked in alcohol, they become stiff, and are, to a certain extent, rendered unabsorbable. Some time ago, he removed from a man's axilla a number of caseous tubercular glands, and put in a decalcified bone drainage-tube, attaching it to the edges of the incision with catgut ligatures. Seven days later, when the dressing was removed, it was found that healing had taken place, and that the catgut ligatures and the tube had disappeared. Four months later, the man returned with an opening in the axilla, which was supposed by some to be due to some of the glands having been overlooked. He passed a pair of forceps into the opening, and drew out the bone drainage-tube, which had not undergone the slightest change. Looking up the literature of the subject, he found a case described in which, for the cure of hydrocele, the sac was incised antiseptically, and a decalcified bone-tube introduced. Fifteen months later, the tube was removed unchanged. It follows from this experience that no reliance can be placed on these tubes. He uses exclusively the red rubber drainage-tubes, which he considers better than the black.

He thought that horsehairs can be utilized to such an extent as to enable us to do away almost entirely with the use of rubber tubes. In large wounds hereafter, he should introduce a drainage-tube and a loop of horsehairs, and at the first dressing remove the large tube, allowing the hair to remain. The same thing can be done in the stump after amputation. A number of hairs may be put transversely, and a few be put between the stitches. When the rubber tube is used, he saw no necessity for leaving it after the first serous discharge has ceased; for where a case is being treated aseptically, pus, even if it should form, will be aseptic pus.

DR. PACKARD called attention to a question which had been asked in regard to

THE BEVELLED INCISION.

All have seen the large leather belts used in machinery. The ends of these are joined by a process termed scarfing, which consists in bevelling the two ends of the belt and bringing them in contact in such a way that the joining will not be thicker than the rest of the belt. He has measured some of these scarves which were twenty inches long. This adds much to the strength of the joint. Some ten years ago, he was called to a patient who had fallen while carrying a piece of china, and cut the back of her hand from the knuckle to the wrist. The skin had been divided in a slanting direction, so that there was a large surface of partially divided skin. He brought the two surfaces together, and found that they could be brought in accurate approximation. The wound healed, and left scarce a trace. This led him to think that such an incision could be made use of in surgical operations, and he decided to try it at the first

opportunity. The first case in which he employed it was in operating on a bursa over the patella. He made an oblique incision, although not nearly so much so as he now employs; and after the wound had healed, he could scarcely find the scar. In all those cases in which it is desirable to avoid a scar, this incision will be of service. It also facilitates healing, and, as all pressure made upon such an incision tends to close it the more, the air is excluded, and an antiseptic effect is obtained. After the operation for strangulated hernia, a truss can be applied much earlier than where the vertical incision is employed, for the irritation of the skin will not be so marked.

CHICAGO GYNECOLOGICAL SOCIETY.

Stated Meeting, December 19, 1884.

THE PRESIDENT, H. P. MERRIMAN, M.D.,
IN THE CHAIR.

DR. W. H. BYFORD read a paper entitled

A CASE OF MURAL PREGNANCY.

The history was obscure. The patient, who was twenty-eight years old, had been married seven years, and had one child, six years old. She thought that she became pregnant, for the second time, in February, 1883. In April, she became fatigued and had hemorrhages, which continued until May 9th—about four weeks. October 14th, a discharge of yellow fluid, about one gallon in quantity, occurred. A putrescent, sero-sanguineous discharge followed, continuing three months. January, 1884, a large, brownish mass, with very fetid odor, was expelled. After this event, menstruation occurred until July. In May she was quite large, and had bearing-down pains. She entered the hospital October 6, 1884. She was tapped October 18th, and about four quarts of thick, tenacious fluid, resembling the fluid of an ovarian cyst, was removed. This fluid coagulated, on the addition of nitric acid, and on boiling. Assisted by Dr. R. Filley, a microscopical examination was made, with negative results. The "Drysdale" cell was not found. Laparotomy was performed, and a fetus with placenta was removed without hemorrhage or difficulty. In order to secure perfect drainage, it was considered best to remove the uterus. The operation was performed on October 30th. The patient did not react, but died within twenty-four hours. Prior to the operation the patient was extremely reduced by her protracted sufferings. Dr. Byford, in a similar case at the present time, would elect the vaginal operation. The specimens removed from the woman were exhibited as supporting the diagnosis of mural pregnancy.

This was the second case of mural pregnancy that had come under the reader's observation within a period of five years. In the first case reported to the Society the patient was in labor and moribund when Dr. Byford saw her. She had been in labor until exhausted. There was no difficulty in making a diagnosis. The head was low down in the pelvis, almost on the perineum. The os uteri was well-nigh inaccessible behind and above the symphysis. The body of the uterus, somewhat enlarged, could be felt in the lower and anterior part of the abdomen, attached to the tumor containing the fetus. The fetus could be felt through the

abdominal walls, surrounded by a thick involucre, apparently as thick as the uterine walls. Fœtal extremities could be distinguished. When dissected, the sac in which the fetus was contained was found to contain a thick layer of muscular fibres. These fibres were directly continuous with those of the uterus. The tubes and ovaries lay on either side of the lower portion of the sac. The fecundated ovum had made its way down the tube, became lodged in a diverticulum in the uterine wall, and from this point of departure was gradually extruded into the cavity of the abdomen. The fetus was thus developed within the uterus, though not within the uterine cavity. The resemblance to normal pregnancy is great, in the presentation and position of the fetus, deep down in the pelvic cavity, behind the vagina. The head, in this case, was fixed by the concentric contraction of the uterine fibres by which it was surrounded, and could be easily outlined as it lay there covered by the posterior vaginal wall.

The specimen presented is much less perfect than the one described because of the numerous effects wrought upon it, during the great length of time it remained in the maternal body, and the mutilation consequent upon enucleation.

The treatment of these cases ought to be considered apart from that of extrauterine pregnancy at term. It is always a matter for special consideration, in connection with each case, as it presents itself, whether or no the removal of the fetus, at term, in extrauterine gestation, should be attempted. The dangers of laparotomy are greatly increased by the inability to remove the placenta. The surface to which it is attached has no contractile power, so that the divided vessels are left patulous. If hemorrhage does not immediately prove fatal, the blood is a source of sepsis, that must almost certainly destroy the patient. Laparotomy would more likely prove successful if performed some days after the death of the child. In these cases of ectopic or interstitial uterine pregnancy the fetus may be easily removed through the vagina. An incision made through the posterior vaginal wall would completely uncover the presenting part, and enable one to apply the forceps, or attack it with the perforator and crotchet, as in ordinary labor. After the removal of the fetus the placenta should be allowed to separate spontaneously. Since writing this report, Dr. Byford has seen a case, reported in the *Annales de Gynecologie*, July, 1884, occurring in the practice of Mr. Matheson, of England, illustrative of the execution of this plan. The case was reported to the London Obstetrical Society under the title, "Extrauterine Pregnancy; the Extraction of a Living Fœtus through the Vagina." The child was slightly asphyxiated, but survived. A sponge, saturated with perchloride of iron, was introduced into the sac after removal of the placenta. The mother recovered. It would seem that the author did not suspect his case to be one of interstitial pregnancy. During the discussion that followed only one of those present expressed the opinion that it was of that variety. Mr. Griffith thought it was either interstitial pregnancy or one in which the fetus was developed in one portion of a double uterus.

DR. EDWARD SAWYER thought that interstitial pregnancy meant the development of the ovum in the uterine portion of one of the tubes. In Dr. Byford's

case the uterine portions of the tubes were not involved. It reminded him of a case he had seen near Denver. In this case a secondary uterus, with muscular walls, had been developed, but as the tubes were not involved he did not feel justified in designating the case one of interstitial pregnancy.

DR. D. T. NELSON said, with reference to the treatment of the placenta, that Dr. Byford's advice was that usually recommended in the text-books. The placenta should be left alone in those cases in which the walls of the secondary uterus were not muscular. He had seen a case, in the museum of the Chicago Medical College, in which no muscular fibre could be detected in the walls. When the walls of the adventitious uterus were muscular it was questionable whether or no the placenta should be left alone. If the placenta is removed, there is danger of hemorrhage; if the placenta remains, there is danger of sepsis. When there was reason to suppose that contractions of the adventitious uterus would check hemorrhage, he thought the placenta should be removed. He had had no experience in these cases.

DR. E. C. DUDLEY said that women, in cases of extra-uterine pregnancy, in which the placenta has been allowed to remain, do not die of sepsis. He had seen two or three cases in which the sac had been united to the abdominal incision. Whenever evidence of sepsis occurred, the sac was washed out and the temperature immediately fell to the normal. The placenta, under these circumstances, is spontaneously eliminated in about three weeks. It required phenomenal powers of diagnosis to tell in the concrete case whether or no the sac had sufficient muscular fibres to prevent hemorrhage. The placenta should be permitted to remain within the sac.

DR. J. H. ETHERIDGE thought that if, on microscopic examination, it was found that the muscular fibres of the normal uterus were continuous with those of the adventitious uterus, the case was one of mural pregnancy. In cases of abdominal pregnancy, there was a line of demarcation between the normal and adventitious uterus.

DR. A. REEVES JACKSON thought, however, with Dr. Sawyer, that the results of the anatomical investigation did not support the author's diagnosis. The uterine portions of the tubes were not involved. So valuable a specimen deserved very close microscopic and macroscopic examination. It ought to be referred to a competent pathological anatomist.

DR. JOHN BARTLETT thought the ovum had not passed through the tube, but had been developed in the broad ligament, beneath the peritoneum, and had, in this manner, derived muscular fibres from the uterus.

DR. W. W. JAGGARD referred to the fact that, next to ovarian pregnancy, interstitial pregnancy was of most infrequent occurrence. Up to the present time about thirty cases, in regard to which the diagnosis was positive, had been reported. Interstitial or mural pregnancy included other sites of development than the uterine portions of the tubes. Dr. Gilbert's case, reported in the *Boston Medical and Surgical Journal*, March 3, 1877, and referred to by Prof. Lusk in his treatise on *Midwifery*, was a case in point. The ovum in this case was developed in what seemed to be a bifurcation of the Fallopian tube. In Dr. Byford's case the tubo-uterine orifices were not involved. The sac

was extrinsic to the uterine walls. It was probably a case of abdominal pregnancy, in which the ovum became attached to the posterior uterine wall, and derived muscular fibres from the locality. The fact that a continuity of muscular fibres from the normal uterus to the adventitious uterus might be ascertained upon microscopical examination, would prove nothing as to the nature of the pregnancy. Dr. Byford's case resembled that of Janvrin, in which the ovum lodged on the posterior uterine wall, and developed in this situation, involving the posterior wall in its sac. The specimen was worthy of more exact investigation, and should be placed in the hands of a competent pathological anatomist.

DR. SAWYER said that abdominal pregnancy, with location of the ovum on the posterior uterine wall, is not at all improbable. He then referred to Bischoff's and Leopold's observations and experiments with relation to the "external wandering over of the egg." Beigel had ridiculed this idea. It was like a blind man introduced into a large empty room, with a thread in his hand, seeking to find and thread the eye of a needle located in some indefinite quarter of the room. Notwithstanding this sarcasm, the fact of the external wandering over of the egg was a matter of positive knowledge. The egg may pass from one ovary to the opposite Fallopian tube, through the abdominal cavity. He thought the specimen exhibited was one of abdominal pregnancy.

DR. DUDLEY thought the fact of the external wandering over of the egg was not disputed at the present time. Playfair, in his treatise on *Midwifery*, gave a clear exposition of the subject.

DR. EARLE said that the fact of external wandering over of the egg was fully recognized twelve years ago.

DR. SAWYER said the ovum in abdominal pregnancy might be attached to the posterior wall of the uterus, the mesentery, under surface of the liver, or to other viscera.

DR. NELSON made the remark that in both of the cases cited by Dr. Byford, *decidua* had been cast off by the *uteri*.

DR. JACKSON said that Fränkel was of the opinion that the formation and extrusion of a decidua was a constant occurrence in extrauterine pregnancy. It was pathognomonic of the condition.

DR. W. H. BYFORD was not surprised that certain members did not agree with him in his diagnosis. He thought that in the first case the fecundated ovum passed through the tube, but had found some diverticulum in the uterine cavity, and had passed into the posterior wall; had developed in this region, pushing the wall before it. Some of the reasons for this position were as follows: The muscular elements of the sac were directly continuous with the uterine muscle. He did not believe that such a muscular sac could develop adventitiously in the abdominal cavity. He had seen cases of abdominal pregnancy in which no muscular fibre could be detected in the sac. The head presentations, deep down in the pelvic cavity, in the direction of the resultant of the forces developed by uterine contractions, supported his view of the case. It is not necessary for the production of mural pregnancy that the tubes be involved. He thought there was much in the remarks of Dr. Nelson and Dr. Dudley. In cases

in which there was sufficient contractility, it was best to remove the placenta. Even under these circumstances, it was not absolutely necessary. There was no danger in allowing the placenta to remain. Finally, he was very positively of the belief that the two cases referred to in his paper were examples of mural pregnancy. The peritoneum was a boundary line between mural and abdominal or peritoneal pregnancies.

DR. SAWYER asked the question, Is the peritoneum a boundary line of importance in the macroscopical or microscopical differential diagnosis between abdominal and mural pregnancies?

DR. JAGGARD, in reply, said that the peritoneum was no barrier. What was the peritoneum? Dr. Etheridge, in an article on "Chronic Adhesive Perimetritis," published in a recent number of the *Chicago Medical Journal and Examiner*, had ably sketched the anatomy of this membrane. It was developed out of connective tissue, according to Rindfleisch and other distinguished anatomists. It offered absolutely no barrier to the attachment of the ovum to the posterior uterine wall, and its development in this situation, with the derivation of muscular elements from the normal uterus.

On motion, Drs. Byford, Merriman, and Jaggard were appointed a committee to select a competent pathological anatomist, who did not belong to the Society, to examine the specimen and report at the next meeting.

NEW YORK SURGICAL SOCIETY.

Stated Meeting, Dec. 23, 1884.

THE PRESIDENT, ROBERT F. WEIR, M.D.,
IN THE CHAIR.

DR. W. T. BULL presented a patient showing the result of

EXCISION OF THE CONDYLE AND NECK OF INFERIOR MAXILLA FOR ANKYLOSIS.

A boy of 14 had been perfectly well until four years ago, when he had an attack of otitis media of the right ear lasting about six months. During this time it was first noticed that he could not fully open his mouth, and this difficulty increased until about three years ago, at which time Dr. Bull began treatment, various apparatus being used forcibly to open the mouth but with only temporary improvement under ether. Twice the adhesions have been ruptured, and stretching by means of gags and plugs kept up for several weeks. On ceasing treatment the stiffness increased. The treatment was very painful.

On admission to the New York Hospital, Oct. 22, 1884, the boy was well nourished and robust in appearance. The jaws could be depressed only so far as to show an interval of a quarter inch between the middle incisors. The left temporo-maxillary articulation appeared normal. The right was the seat of thickening and induration of the tissues overlying it. These formed a swelling the size of an almond, which was not tender.

Oct. 23, 1884, the operation was performed. An incision one inch and a half long was made just below and parallel with the right zygomatic arch. A second vertical incision three-quarters of an inch long was made downward from the first at its posterior third. These were carried down until the condyle and eminentia

articularis were exposed. The condyle was thickened, and the joint ossified, at least the only indication of its existence was a thin layer of cartilage a quarter inch long. The periosteum was pushed back, and the condyle and neck removed with chisel, gouge, and roughened forceps. This was a tedious procedure, owing to the depth and narrowness of the wound. The jaw was then forcibly depressed with a gag, rupturing some adhesions in the soft parts, and its motions were perfect. The wound was irrigated with 1 to 1000 solution of bichloride of mercury, and closed with catgut sutures save at the lower limit, where a small bone drain was inserted. A temporary dressing of bichloride gauze was firmly applied.

24th.—A.M., 99°. A dressing of iodoform gauze next the wound, over this peat and cotton, was applied. P.M., temperature 99.8°.

29th.—Pulse and temperature normal since last note. Dressing removed. Union complete save at site of tube, where a small ulcer remains. Tube had been almost wholly absorbed. Patient has no pain and opens his mouth freely, eating regular ward diet.

Nov. 5.—An ulcer size of split pea remains. Patient opens jaw so that middle incisors are one and one-sixteenth inch apart. No pain.

Dec. 23.—There is the same freedom of motion as before noted. The cicatrix is represented by a depression vertical in direction, of one-half inch in length.

COMPOUND FRACTURE OF INNOMINATE BONE; OPENING HIP-JOINT; RECOVERY WITH PERFECT USE OF LIMB.

DR. W. T. BULL also presented a patient with this history.

Three years ago this man, a laborer, thirty years of age, was admitted to the Chambers Street Hospital half an hour after having fallen through a hatchway into the hold of a ship, a distance of twenty feet. He struck with right side against a pair of ice-tongs, the points of which made a wound below and behind the anterior superior spine of the ilium. The finger introduced into this found the muscles lacerated, the bone behind and below the anterior superior spine comminuted, and a fissure extending, it was thought, into the acetabulum, the capsule of the hip-joint was torn close to the acetabulum, and the cartilaginous head of the femur could be felt. There was profound shock. A few hours later the wound was enlarged, and several loose pieces of bone removed. They were all fragments of the outer surface of the ilium, the largest was three inches long by one and a quarter inch broad.

The wound was thoroughly irrigated with a five per cent. solution of carbolic acid, two large drainage-tubes were left in the wound, which was left open, and two other tubes were inserted through counter-openings, behind the trochanter and near the tuberosity of the ischium, one of the tubes leading to the site of the opening in the capsule. Compresses of two and a half per cent. carbolic acid were put over the hip, a weight and pulley extension applied—the tubes washed through four times a day with two and a half per cent.—and appropriate general treatment ordered.

From the third to the seventh day there was high traumatic fever, temperature 102½° at night, with delirium, diarrhoea, and prostration. The irrigations were made every two or three hours, and the discharge was

abundant but not decomposed. After this improvement was slow but steady, and without other complications than the formation of one or two pockets, which gave rise temporarily to fever. The extension was kept up for four weeks. The tubes were shortened from time to time, and the irrigations were made much less frequently. The man walked on crutches at the end of the third month, with considerable stiffness at the hip, and the wound was reduced to one sinus, through which several fragments of bone came away subsequently. At the end of the fourth month he had quite regained his strength, and could walk with but little stiffness. He resumed work after the end of six months—but the sinus did not close till several weeks later. He had had no further trouble from the injury, and thinks the functions of the right leg perfect. It is now three years since the injury, and I find the limb half an inch smaller in circumference at the upper part of the thigh, and one-quarter inch smaller at the lower third, than the left. The motions of the joint are free, except that of external rotation, but the hindrance is not sufficient to attract his attention. There is flattening of the soft parts between the anterior spine and the trochanter, and the brim of the pelvis for four or five inches behind the anterior superior spine is irregular in outline. The cicatrices are soft.

DR. GEORGE A. PETERS then read a paper entitled
CASES OF EXCISION OF SUPERIOR MAXILLA, WITH
REMARKS.

(See next number of THE MEDICAL NEWS.)

DR. H. B. SANDS said that the main points of the paper were scarcely debatable, as the Society would doubtless endorse most of the opinions expressed by Dr. Peters. But, if he had not misunderstood the author of the paper, he had recommended

THE ADMINISTRATION OF ETHER BY THE RECTUM

as a safe and useful method. Dr. Sands had had no experience in this mode of producing anæsthesia, which had not impressed him favorably.

But, as many of the members of the Society had had such experience, he would like to inquire whether their verdict would sustain the position taken by Dr. Peters. Judging from what he had learned concerning etherization by the rectum, Dr. Sands had concluded that it was far from being a desirable procedure, as it was attended with risks which are difficult to avoid, and almost impossible to estimate. He had the privilege of witnessing the operation described in the paper; and, being only a spectator, he was perhaps able to observe better than those who were engaged in performing the operation the effect of the anæsthetic. It seemed to him that the ether was but little under the control of the administrator, and that, in spite of the precautions used, a very large accumulation took place in the intestine, with the effect of producing cyanosis and very deep anæsthesia for a period which made a bystander feel anxious with regard to the result.

Dr. Sands would hesitate from what he had observed in this case, even in operations of the kind mentioned in the paper, to adopt this method as a substitute for inhalation in the ordinary way. His own practice, when employing anæsthetics in the operation for excision of the superior maxilla, had usually been to avoid inducing

profound insensibility in the later steps of the operation. By adopting this precaution, by operating without undue delay, and by causing the head to be inclined forward whenever hemorrhage was copious, he had always been able to prevent the entry of blood into the air-passages, without the necessity of plugging the trachea. Patients treated in this manner sometimes showed signs of pain, especially when the larger nerves were divided; but afterward they had no recollection of having suffered during the operation. Dr. Sands believed that in all surgical procedures accompanied with the risk of escape of blood into the larynx, deep anæsthesia should be avoided. Regarding the general administration of ether by the rectum as a substitute for the usual method by inhalation, he would like to know whether the advantages were not outweighed by the risks.

DR. W. T. BULL said that he had considerable experience with etherization by the rectum, the results of which he had already published. He had administered ether in this manner in seventeen cases, and had reached the conclusion that the method should not be practised, because in a large proportion of those cases the reaction which followed, on the part of the intestinal tract, was very considerable. In some cases there was diarrhoea, with bloody passages, and in others ordinary serous diarrhoea, and the diarrhoeal discharges seemed to occur without very much reference to the apparent good general condition of the patient. In one case the diarrhoea continued for two or three days after the operation. Judging from his own experience, he was unable to call etherization by the rectum a safe procedure, and this conclusion had been confirmed by the occurrence of deaths, in different hospitals, from ether administered in this way, and when there was nothing in the nature of the operation which would have imperilled the life of the patient. In the operation in question, he should prefer to trust to the method mentioned by Dr. Sands, rather than resort to etherization by the rectum.

DR. L. A. STIMSON remarked that he had already presented to the Society an instrument devised for continuing etherization during the operation described by the author of the paper. The instrument consisted of a large tube, about the size of a No. 40 urethral catheter, surrounded at one end by a large sponge with a rubber coating, which, when passed into the pharynx, prevented the flow of blood in that direction while the patient breathed through the tube. He had successfully used this apparatus in two cases.

DR. PETERS said that his feeling concerning etherization by the rectum was such as had been stated by Dr. Sands, unless the method could be surrounded by safeguards in the administration which would deprive it somewhat of its dangerous element. The results in the two operations described in the paper, performed upon a single patient, were eminently satisfactory. The patient was profoundly anæsthetized, and the anæsthesia continued for some time, but he came out from under the influence of ether with less subsequent annoyance than usually follows the administration of this anæsthetic. The patient himself was so much pleased with the mode of administration that at the second operation he requested that the same method be employed as was adopted at the first operation, a point which, of course, went only for what it was worth. Like the administration of ether by any method, it was

important to conduct the procedure very carefully. From the experience with these cases, Dr. Peters stated that he should give this method further trial.

DR. BRIDDON had employed Nussbaum's method with much more pleasant results than any other he had employed. The method consists, first, in the administration of a full dose of morphine hypodermically, ten or fifteen minutes afterwards administering the anæsthetic, then making an incision into the trachea or larynx and introducing a tube with a funnel-shaped extremity, over the end of which is secured a piece of flannel, upon which chloroform can be dropped, using it in preference to ether. In the mean time the fauces are packed to prevent blood from entering the air-passages. The quantity of chloroform sufficient to maintain anæsthesia under these circumstances is exceedingly small; usually only a few drops being required. He had seen anæsthesia produced by the rectum in a number of cases by his friends, and he thought their experience was similar to that related by Dr. Bull, and that none of them would adopt the method at the present time.

THE PRESIDENT remarked that his own experience with rectal etherization was checked early by an unfortunate case in which death resulted. It was true that in that particular case, which had been published, some errors in the mode of administration were made. Since then he had administered ether in this manner four times; in two cases the method worked satisfactorily, and in the other two cases there was some diarrhoea with dysenteric stools. He had reached substantially the conclusions already expressed by other members, and it was, to his mind, a method attended with considerable risk; too great a risk to be lightly resorted to. He thought that the opinions already expressed had been confirmed by reports from Philadelphia and Boston, and also in the English journals. The method of managing the anæsthesia mentioned by Dr. Sands had suited him best in the operation of extirpation of the upper jaw.

DR. L. A. STIMSON presented a

SPECIMEN FROM A CASE OF OVARIOTOMY,

with the following history: A patient, 43 years of age, in good health, a widow, the mother of one son eighteen years of age, first noticed a tumor in the left iliac fossa five years ago. It steadily increased in size, and she finally sought relief, by operation, from the distress occasioned by the weight and pressure of the tumor. There had been no disturbance of the menstrual function. Dr. Stimson operated on the 17th of December, by making an incision a little to the left of the median line, midway between the umbilicus and pubes. On account of the thickness of the abdominal parietes the incision ultimately reached four inches in length. When the peritoneum was exposed it was divided with scissors to the extent of about one inch. A trocar was then inserted into the tumor; two and a half gallons of rather thick, opaque, brownish-gray liquid were withdrawn. The tumor was grasped with the volsella forceps, drawn out through the incision, and it was then ascertained that it had no pedicle, but had developed between the walls of the broad ligament, and was continuous with the left side of the uterus. The uterus was drawn up into the opening, an incision made

into the posterior wall of the sac, and an attempt made to enucleate the sac. After this had been carried far enough to make it probable that enucleation could be completed, a stout catgut ligature was placed, by transfixion, about that portion of the mass which corresponded to the left Fallopian tube, and the upper portion of the sac was then cut away; enucleation of the remainder was rapidly completed. The portion of the sac adjoining the uterus was trimmed down and united along the sides of the uterus with a continuous catgut suture; the raw surface of the uterus thus covered in was sprinkled with iodoform. The walls of the pouch left after enucleation were drawn up into the abdominal wound and fixed with sutures to the skin. A rubber drainage-tube was passed into Douglas's pouch, a bone drain into the lower angle of the wound, and a second rubber drain into the pouch. The antiseptic precautions observed were purification of the hands and instruments, bichloride irrigations during the operation, and the use of sponges dipped in a warm carbolic solution after the peritoneal cavity had been opened. Two of the drainage-tubes have been removed, the bone one on the second and the first rubber one on the third day; the drain which entered the pouch of enucleation is still retained, although there is but scant discharge from it. Since the operation the patient has done perfectly well. The sac of the tumor had a rather thick wall, with a rough and granular inner surface. In the portion of the sac cut off, the line of incision passed through a number of small cysts, which occupied the wall of the sac and which contained a brown and rather viscid liquid.

In response to a question asked by the President, DR. STIMSON said he made the incision a little to the left of the median line for two reasons: first, he wished the parietal wound to include the muscular layer; and, second, because the tumor was situated upon the left side, and a lateral incision, he thought, would somewhat facilitate the operation upon the pedicle.

DR. BRIDDON presented a specimen of

DISLOCATION OF THE THUMB FORWARD,

which occurred nineteen years before the death of the patient. It was referred to a committee, consisting of Drs. Briddon and Stimson, for examination and subsequent report.

LAPARO-HYSTERECTOMY FOR UTERINE FIBROIDS.

DR. WEIR showed a specimen of uterine mural fibroids weighing eleven and a half pounds, which he removed by abdominal section at the New York Hospital, November 21, 1884, from a single woman, 45 years of age, who was much exsanguinated by hemorrhage of five years' duration. The operation was conducted in a room filled with carbolic spray, though none was directed upon the seat of operation. The incision required was nearly eight inches long, and just permitted the emergence of the tumor, which was irregular in shape; being, in fact, trilobed. The broad ligament, with the ovaries, was tied off by silken ligature, and then divided down to the neck of the uterus. Thornton's clamp or *écraseur* was temporarily passed around the mass to make a groove for the elastic ligature, which was then tied tightly and the parts above removed. About two inches of the cervix were left. This stump was very movable, and could easily be applied to the

lower angle of the wound. It was secured there by a long needle, which traversed it and rested with its free ends on each side of the abdominal wound. The parietal peritoneum was sewn about the pedicle; first to the skin, subsequently to the peritoneum of the pedicle, so as to shut it off from the general peritoneal cavity. The remaining portion of the wound was closed as usual, the peritoneal edges being sewn, in addition, by a continuous catgut suture. The pedicle was dusted with iodoform, and peat bags applied to keep the parts as dry as possible. The patient did well for three days, when elevation of the temperature coming on, 103° F., with distention of the upper part of the abdomen, a rubber coil was applied to the belly. The next day, repeated bloody evacuations from the bowels took place, which continued with normal temperature until November 27, when she died. The autopsy showed no inflammatory action in the abdominal cavity, nor in the abdominal walls. Micrococci were found in the kidneys, which were in a condition of chronic interstitial nephritis. The quantity of urine passed subsequent to the operation was satisfactory. In addition to the uterus removed, specimens showing the line of union of the wound and the condition of the stump were exhibited.

DR. HUTCHISON inquired concerning the usual behavior of elastic ligatures when left in the abdominal cavity.

DR. LANGE said it seemed that in most cases the rubber ligatures became encysted when left in the abdominal cavity, and gave no subsequent trouble. In his last case, however, about eight weeks after the operation the patient began to have a discharge, and the ligature with the stump of the uterus came away through the vagina. At that time the patient had passed from under his observation. Subsequently he saw the patient; and she was in a very comfortable condition, although there was still some discharge from the vagina, but he was unable to detect the opening through which the ligature and the stump of the uterus had escaped. It seemed to him that they must have passed through the cervical canal. He might also mention that the ligature was applied very deep, immediately above the fornix of the vagina, and perhaps this immediate proximity to the vagina had something to do with the discharge of the ligature through the cervical canal.

DR. W. T. BULL had had a similar experience with regard to the silk ligature after removal of the uterus, with the ovaries, for a large subperitoneal uterine fibroid. Six months after the operation the patient returned, and said that a discharge from the vagina had persisted since about two months after the operation, and on making a vaginal examination he saw a piece of silk, hanging from the os, which he seized and withdrew, and found to be the entire ligature.

In cases in which the uterus is made the pedicle, the fact of the liability of the escape of the ligature and the stump through the vagina, had been used as an argument for the external treatment of the stump; at all events, it was one of the features of the intraperitoneal treatment which was undesirable.

DR. LANGE remarked that it was difficult to see how one could secure a deep situated pedicle and treat the stump with the ligature through an external opening.

DR. BRIDDON thought that no definite rule could be established, but that each case should be decided by

itself, and chiefly by the length of the pedicle. He thought that the manner of escape was easily explained by the results of experiments which had already been reported in Sir Spencer Wells's book, namely, that the sutures shut off entrance to the peritoneal cavity, adhesions between the peritoneal surfaces occurred within a few hours, and if ulceration went on, the more direct way of escape was into the vaginal cavity.

DR. W. S. HALSTED exhibited

A NEW NEEDLE HOLDER,

for holding needles flattened from side to side instead of antero-posteriorly. The especial advantages which it possessed were, that it could be taken apart readily and cleaned, and it did not allow the needle to rotate and slip.

CORRESPONDENCE.

INTERNAL MEDICINE IN THE VIENNA HOSPITALS.

To the Editor of THE MEDICAL NEWS.

SIR: Certain features render Vienna, of all places, perhaps, the most attractive to physicians seeking experience and information in the various branches of medical science. They are, namely:

1. That into or about one great hospital is gathered by far the larger portion of the vast clinical material of the city.
2. That the patients here are thoroughly utilized for purposes of instruction, suffering, meanwhile, no detriment themselves thereby.
3. That the instruction given is of the very best practical kind.

That this is no overestimate of the advantages to be found here is proved by the presence of a very large number of foreign physicians from almost every land. Of these, very many are subjects of the South American States, Great Britain, and Germany; but by far the greater portion comes from the United States of America, which country contributes probably over one hundred physicians, including about ten women.

The method of instruction is by clinical and didactic lectures given by the many professors, and by the University courses and private courses given by them, and the very numerous "docents." The number of these courses advertised on the bulletin boards of the hospital is indeed so great as to be absolutely bewildering to a new-comer.

The University year is divided into two terms or "semesters," in which time are held the regular lectures, clinics, and courses; and, entirely independent of them, the private courses. During the holidays many of the latter still continue, so that plenty of instruction can be procured throughout the entire year. The duration of each private course is from four to six weeks. The University courses continue the entire term.

At 7 A. M. throughout the summer semester, and at 8 A. M. during the winter, begin the two clinical lectures on internal medicine by Professors Bamberger and Nothnagel. That of Nothnagel is the one more sought after by the foreign physicians.

The oftener one hears him the more does one admire

the distinguished clinician's evident mastery of his subject, and great ability as a lecturer.

The case having been brought before the class, one of the course students is called upon to examine it. He is requested first to interrogate the patient as to the nature of his disease. As the questions asked are frequently not much to the point, the lecturer supplements them by questions of his own, and then requests the assistant to read the clinical history. He next reiterates any matters of importance discovered from the history or by the questioning, and asks the examining student what different diseases could be indicated by the existing symptoms. Now, the direct examination of the case is commenced. No point is omitted. The student is asked to notice and describe the color of the face and mucous membranes; the condition of the pupils, of vision, and of hearing; the state of the sensorium; the condition of the glands of the neck, and the presence of any visible pulsation in the vessels there; the inclination of the clavicles; the shape of the thorax and abdomen, and all characteristics and peculiarities to be found there; the amount of bony, muscular, and adipose development; the position of the body, etc.

Nothnagel evidently lays great stress upon inspection, and it is often surprising what valuable points of diagnosis he develops by it. He next gives the contents of the thorax and abdomen a most thorough examination by means of the remaining physical methods. It is worthy of note that he uses a percussion hammer and pleximeter. (The same is true, by the way, of Frerichs, Guttman, and others in Berlin.) The frequency of the respiration and of the pulse is always counted in the presence of the class, and the examining student required to describe the character of the latter. The temperature is very often measured in the lecture-room and the urine always examined there. In cases of suspected pleuritic effusion, the "puncture test," by means of the hypodermic syringe, is also conducted before the students. Nothnagel depends almost entirely upon the reaction with potassium ferrocyanide and acetic acid in testing for albuminuria, believing it to be delicate, reliable, and undoubtedly more convenient than that with nitric acid and heat. The record of the temperature for several days before is announced, and the sputum, if existing, is exhibited, and the result of any previous search for tubercle bacilli reported.

Having finished the examination, he enters upon the diagnosis of the case. All possible diseases capable of giving rise to any of the prominent existing symptoms are taken into consideration, and are one by one excluded, until the final diagnosis is attained. The lecture has been meanwhile delivered largely at the examining student, and interspersed with numerous digressions into the realms of medical diagnosis, and with frequent questions asked of the student involving his knowledge of anatomy, physiology, *materia medica*, and chemistry; with all which he is presumably familiar, having usually already passed examinations in them. Unfortunately, however, the German students share the fallibility of other men, and sometimes return surprising answers. One of them recently made the reply that calomel was a vegetable. Another, that potassium iodide was the form in which iodine was usually used as a counter-irritant; and that the prominent symptom of cantharis poisoning consisted in muscular cramps.

Another, that during sudden anæmia of the surface of the body the blood is collected in the heart.

The digressions already referred to form one of the most interesting and instructive features of Nothnagel's clinics. For example, while lecturing on pneumonia, he may give a complete review of the varieties of cough, with their diagnostic indications. Or he may sometimes speak at length of sputum, chill, urine, etc.

Next in order the lecturer talks of the prognosis, first asking the opinion of the examining student; and finally of the therapeutics, although comparatively little is said on the subject.

He speaks rapidly and with vivacity, and holds the full attention of the class, although he often spends from one to two hours over a case. He delivers his lectures in a chiefly systematic course, being enabled to do this by the large amount of clinical material at his command.

Bamberger calls upon two students for each case; one to examine the lung, the other the heart and abdominal organs. He sits while speaking, and seems to address himself principally to the *bed*, inasmuch as he scarcely ever glances at either the patient, the examining students, or the class. His voice is very low and quiet, and it is almost impossible for a foreigner to understand him, unless seated very near the front. His method is just as thorough as Nothnagel's, but perhaps not so well calculated to instruct. He reads to the class the entire recorded clinical history, with the physical signs as already found; then, stating at once the diagnosis, he discusses thoroughly the usual symptoms and course of the disease. A physical examination of the patient is then made by the students, who, while conducting it, are questioned by the professor as to its results. Bamberger next carefully investigates the case himself, and then considers the prognosis and therapeutics.

As further distinguishing his clinical methods from those of Nothnagel, can be noted that neither pleximeter nor percussion hammer are employed; that all normal or abnormal areas of percussion dullness are invariably mapped out on the body of the patient with the aniline pencil; that the femoral artery is always ausculted in any case of suspected valvular disease of the heart; that the urine is examined for albumen by the heat and nitric acid reaction only; and, further, that whereas the symptoms and course of the disease *in general* are very thoroughly discussed, its differential diagnosis in the case before the class is not so clearly elucidated.

But what to the foreign physician is of far greater value than the lectures of the professors are the *private courses*, of which the favorite ones, on Internal Medicine, are those given by Drs. Toelg and Von Jaksch, the first assistants respectively of Bamberger and Nothnagel. The members of these courses have the opportunity of personally studying in the wards many most interesting cases. A patient is assigned to two physicians who carefully examine him, and form their own diagnosis of the disease. The assistant then thoroughly discusses the case, and approves or criticises the diagnosis. They endeavor to teach the newest and best methods of medical diagnosis, including also the examination of sputum for tubercle bacilli, of the blood for any microscopical changes in it, and of the urine for any chemical or microscopical alterations.

Toelg's method of demonstration is wonderfully clear;

that of Von Jaksch thoroughly scientific. With the former, the physician studies his own case only; with the latter, he has the opportunity of examining those belonging to the other members of the class, and of hearing the docent's discussion of them. As these courses are in great demand, and are limited to eight or ten men, it is often necessary to make application months in advance.

The hospital offers numerous other courses in this branch of medical science, some excellent; as, for example, those of Dr. Neusser, Dr. Heitler, and Prof. Schrötter. Schrötter was an assistant of Skoda, and, it is said, follows his methods of demonstration. He shows clearly how the sounds of percussion and auscultation can be produced artificially, and then exhibits cases to illustrate them.

Any one often in the wards for Internal Medicine will notice many interesting characteristics of diseases, and their treatment there. The first and most striking of these is the dreadful prevalence of tuberculosis. So common is it, indeed, that it goes by the name of "*Morbus Viennensis*" as frequently as by any other. The possibility of its causing or complicating other diseases must be always considered here. Primary pleurisy in the Vienna hospital is comparatively so rare that a case of it was recently exhibited as almost a curiosity. Unfortunately for the patient, the effusion obstinately resisted treatment, and soon evident signs of infiltration of the lung of the other side appeared, thus establishing the disease as probably in this case, too, a secondary affection. In the wards of both Bamberger and Nothnagel the finding of tubercle bacilli is considered proof positive of the existence of tuberculosis. Nothnagel institutes an investigation of the sputum in all suspected cases. He insists, however, that a single examination, in case no bacilli are found, shall not be considered decisive, but that it shall be repeated many times. If, after numerous examinations, none can be discovered, he maintains that the presence of tuberculosis is certainly exceedingly improbable. These conclusions are the result of the careful study of the sputum of hundreds of patients.

As common as is tuberculosis, almost so rare appears to be typhoid fever, of which comparatively few cases are to be found in the hospital. It was formerly as prevalent in Vienna as in other localities; but on the introduction, in 1873, of the present water supply, coming from the Styrian Alps, a notable decrease in the frequency of the disease took place.

Noticeable also is the largely expectant plan of treatment followed in many diseases. Cases of pleurisy and pneumonia receive usually a purely symptomatic treatment. In impending heart-failure, in the latter disorder, dependence is placed on large doses of alcoholic stimulants. The employment of diuretics or purgatives in pleuritic effusion is almost entirely discarded, and the fluid either trusted to nature, or, when necessary, aspirated. Acute Bright's disease and typhoid fever are also treated purely expectantly. Cold applications were made in those cases of inflammation in which hot would generally be employed in America. The hypodermic syringe is very freely used as a means of diagnosis of the presence and nature of fluid contained in the pleural or abdominal cavity.

Neither cod-liver oil nor iron is given in phthisis or

other diseases when there is accompanying fever. The temperature is often recorded every two hours in cases in which it is likely to rise above the normal. A daily record of the quantity of urine passed and its specific gravity, and of the number and character of the stools, is noted on the chart above each bed. The cold bath treatment of fever is not much employed; but as an antipyretic, quinine is used in the wards of Bamberger, and in those of Nothnagel is at present given *thallin*, a most valuable agent, whose properties were recently described by Von Jaksch, before the Wiener K. K. Gesellschaft für Aerzte. In diabetes mellitus a strictly non-amylaceous diet is not adhered to, and no reliance is placed on medication. If the patient can be sent to Carlsbad, strong hopes are entertained of a cure, or of great amelioration of all the symptoms. Catarrhal icterus is generally treated with Carlsbad water. The doses of morphia are usually quite small, about one-tenth of a grain being the amount given hypodermically. The prognosis of the disease is always spoken in Latin in the presence of the patient, and death called "*Exodus lethalis*." Latin, indeed, is very commonly used whenever it is desired that the patient be kept in ignorance of the remarks made.

Closely connected in many branches with the subject in hand, is that of the diseases of children. These, although well taught, do not offer so many opportunities for study as could be desired. The two principal clinics are those held by Prof. Monti in the Polyclinic, and by Prof. Wiederhofer in the St. Ann's Children's Hospital, both institutions situated quite near the general hospital. Monti sits at a table on which the child is placed, and around which the class gather. After eliciting the symptoms, by a careful questioning of the mother, he examines the patient, and then discusses fully the nature and differential diagnosis of the disease, and with particular care the therapeutics. The prescriptions to be employed are written upon a black-board provided for the purpose. The child is then taken about among the students for examination by them. The latter portion of the hour is devoted to a systematic lecture, continued from day to day, upon some selected branch of children's diseases. Much can be learned at this clinic, although no great opportunity is given for personal examination of the cases, as the course is largely attended, and but few patients exhibited daily.

Wiederhofer's clinic is conducted very similarly, except that no systematic lecture is delivered. It has the advantage that many house-patients are shown, as well as those from the ambulatorium. The lecturer states the diagnosis as soon as the child is brought in, and says but little on the subject. He then examines the case, and discusses the symptoms and treatment in an unsystematic and informal, but very interesting manner. An opportunity is given for personal examination of the patient. After the lecture, the class accompany the professor in his visit through the wards. The privilege of attending the very large ambulatorium of the hospital can often be obtained. Here frequently as many as one hundred children can be seen in the course of the morning. Wiederhofer's assistant, Dr. Frühwald, often gives excellent courses. Very good courses are also held by Dr. Fürth in the Polyclinic, and by others elsewhere.

It is, of course, of the greatest practical benefit to a

physician to be able to compare the diagnosis made during the life of the patient with the actual condition of the organs as seen on the post-mortem table. This can be done here to wonderful advantage, and microscopic pathological anatomy studied with great profit.

There is in the general hospital a death rate of about three thousand patients yearly, and on all of these the pathologists have the right to make post-mortem examinations. In point of fact, from eighteen hundred to two thousand such examinations are made, beside the autopsies of the criminal, accident, and similar cases of the city, which are also conducted in the hospital. Autopsies can be witnessed during almost the entire morning, and in the afternoon an excellent private course, limited to ten or twelve members, on the making of post-mortem examinations and the recognition of fresh pathological specimens, is held by Dr. Zeman, the assistant to Kundrat, the professor of pathology. In this there is such a wealth of material at hand, that the question is not what can be found interesting to the class, but what "will keep until to-morrow."

During the first half of the course, Zeman exhibits and explains pathological specimens, and usually during the latter portion the members of the class make their own diagnosis of specimens given them. Throughout the whole course they have opportunities to perform autopsies under the direction of the docent. His method of making autopsies is rapid, complete, and very systematic, and the student's eyes thoroughly trained to notice even slight pathological changes. Those desiring it can attend systematic lectures on pathological anatomy by Prof. Kundrat, and on experimental pathology by Prof. Stricker. There is also a large museum of alcoholic specimens.

Good opportunities are offered for the study of pathological histology, although this is not so well taught as in Heidelberg or Strassburg. At the Rudolph's Hospital, which is unfortunately a long distance from the general hospital, Dr. Weichselbaum gives excellent courses on both macroscopic and microscopic pathological anatomy.

J. P. CROZER GRIFFITH, M.D.

VIENNA, Dec. 24, 1884.

COCAINE IN DYSMENORRHEA.

To the Editor of THE MEDICAL NEWS.

SIR: Having employed the new anæsthetic, cocaine, with the happiest results, in a case of dysmenorrhœa, I thought it might be of interest to publish the facts.

My patient has suffered for years from painful menstruation, accompanying a retroflexion of the uterus, complicated with adhesions. The deformity of the uterus has been rectified by treatment, but the pain during the menstrual period is still as agonizing as ever. The most acute and intolerable anguish is felt in the left inguinal region, and is accompanied by a wind colic that causes the sufferer to writhe in agony. Nausea and vomiting add to her distress, and she seems at times ready to go out of her mind with suffering.

While still hoping to effect a permanent cure of this condition by appropriate treatment, temporary relief by the administration of morphia is all that I have hitherto been able to afford her; at the same time, that drug produces such disagreeable effects, that the remedy is only

not quite so bad as the disease. It excites the nervous system, banishing sleep, it adds to the nausea, increases the prostration, destroys the appetite, and fails to keep off the wind colic. But on the last occasion, instead of morphia, I used cocaine, injecting subcutaneously over the left ovary, at first three, increased afterwards to five minims of a four per cent. solution. Almost immediately after each injection the pain in the inguinal region ceased to be felt, the nausea and wind colic were relieved, and, instead of nervous excitement and wakefulness, a soothing effect inclining to sleep was experienced. Five minims of a four per cent. solution were sufficient to afford complete relief for five or six hours, and comparative immunity for a much longer period.

While, however, the drug acted thus admirably, both locally and generally, it had no effect, apparently, on the bearing-down pains, and pain in the back, but only upon the local pain in the neighborhood of the spot where it was injected. It was also noticed, that, while it relieved the nausea and vomiting of a reflex origin, it sometimes caused slight nausea itself, but this was only temporary. No other unpleasant effect was experienced, if we except a slightly bitter taste imparted to the tongue.

If cocaine can thus be made to supersede morphia in such cases, it will certainly prove an inestimable boon to many an unfortunate sufferer. And if it is able to relieve that excruciating intestinal pain usually called wind colic, which in so many cases is sure to follow the slightest surgical interference with the uterus, this of itself will be no mean trophy added to its many wonderful triumphs over human suffering.

JOHN FORREST, M.D.

CHARLESTON, S. C.

NEW INVENTIONS.

A NEW EVACUATOR

FOR THE REMOVAL OF CALCULOUS DÉBRIS FROM THE BLADDER AFTER LITHOTRITY.¹

BY H. O. WALKER, M.D.,

PROFESSOR OF GENITO-URINARY DISEASES, ORTHOPÆDIC SURGERY AND CLINICAL SURGERY, IN THE DETROIT MEDICAL COLLEGE, AND SURGEON TO HARPER HOSPITAL, DETROIT, MICH., ETC.

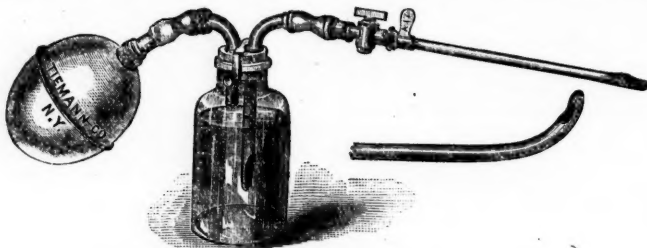
THE instrument consists of a six-ounce wide-mouthed glass bottle, fitted with a rubber stopper having two openings through it for the introduction of two metal tubes, one short, extending down about half an inch below the stopper, and the other long, extending to within one inch of the bottom of the bottle, with an oval (side) opening. These are held in place by a steel bar or flange over the stopper, and fastened by a screw.

The metal tubes are curved at their upper extremities, both having for a medium of connection a short piece of rubber tubing. To the short one is attached a rubber bulb, and to the long one the evacuating-tube, with stopcock and bayonet connections. The evacuating-tubes vary in size, with either curved or straight extremity. No. 26 F. is the one best adapted to general use, and less liable to do injury to the deep urethra. When in use the bottle is to be filled with warm anti-

¹ Exhibited before the Michigan State Medical Society, June 11, 1884.

septic water just to clear the opening of the short tube. After the introduction of the evacuating-tube into the bladder, it will be well, before making the connection with the long tube, to exclude the air. This can be done by forcing water into it by a slight pressure upon

the bulb, and then closing the stopcock. Everything being in readiness, the connection is made and the stopcock opened and gentle pressure made upon the bulb, so as to force out from the bottle about an ounce and a half of water, when by a sudden relief of pressure



upon the bulb a return current takes place bringing with it broken calculus. The repetition of this manoeuvre is kept up until the operation is completed. Experiments which I have made with it, both in a glass bottle and a soft rubber bag, satisfy me as to the ease and rapidity of its action. Another important feature of its working is that no fragments pass back into the bladder after they have once fallen into the bottle. It is entirely under the control of the operator, and does not require an assistant to work the bulb.

I am indebted to Dr. D. C. Wade, of Holly, Mich., for suggestions, and to Mr. Stohlmann, of Tiemann & Co., New York, for the care which he has manifested in the manufacture of the instrument.

33 LAFAYETTE AVE., DETROIT, MICH.

NEWS ITEMS.

NEW YORK.

(From our Special Correspondent.)

DR. RUDOLPH TAUSZKY, who has figured in the courts for some time past as an "expert" in insanity cases, tried to kill his wife, and afterwards fired a ball at his own head, which he wounded slightly. He is now at the Presbyterian Hospital, and the "penny dreadfuls" are advancing all sorts of theories. No one, however, disputes the fact that he is insane.

THE DISPENSARY ADVANTAGES in this city are so extensive that the poorer and sometimes even the middle classes are enabled to get good medical and surgical advice without pay. Since the two institutions for advanced medical learning have been established, there is not enough clinical material "to go around." It is now no common matter to find "interesting cases" hiring themselves out at rentals ranging anywhere from twenty-five cents to one or two dollars per lecture, and if this thing goes on the possessor of a well-marked case—say, for example, of lupus—may regard his "face as his fortune."

CIVIL SERVICE REFORM.—So far as the workings of Civil Service Reform are concerned in this city, there might be an improvement. It is to be regretted that so much red tape entangles them, and that applicants must wait so long before coming up for examination. The fault is not with the medical examiners, but with the other officials.

THE BROOKLYN BOARD OF HEALTH has recently done good work and every precaution has been taken by Dr. Raymond and his assistants to guard against the approach of cholera.

HERMAPHRODITE SHOW.—There is an hermaphrodite who exhibits herself (?) in a down-town street, her chief patrons being brokers and fast men about town. She has been facetiously named "Madame Duplex," and is coining money as the result of the prurient curiosity of her many visitors.

THE ENGLISH CHOLERA COMMISSION.—DOCTORS KLEIN and GIBBES have completed their investigations, and have submitted a short preliminary report. They state that Koch's comma-bacilli are not peculiar to cholera, but occur in other intestinal diseases; that these bacilli do not behave in any way differently from other putrefactive organisms; and that inoculations of animals with recent and old cultivations of comma-bacilli, as well as with cholera dejecta, produce no effect.—*Med. Times and Gazette*, December 13, 1884.

CHOLECYSTOTOMY.—We learn that DR. CHARLES J. PARKES performed cholecystotomy, on January 19th, on a female, aged 35, who suffered from enlargement of an uncertain nature in the region of the gall-bladder, which, owing to the fact that she had passed gall-stones, led to the belief that it was due to impacted gall-stones. An exploratory incision was made under strict antiseptic precautions, the gall-bladder was penetrated, emptied of its contents, and stitched to the edges of the wound. No gall-stones were found, but the bladder was distended with clear mucus. The day following the operation, the case was doing well.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM DECEMBER 30, 1884, TO JANUARY 5, 1885.

GIRARD, A. C., *Captain and Assistant Surgeon*.—Ordered from Department of Missouri to Department of the East.—*S. O. 304, A. G. O.*, December 29, 1884.

APPEL, A. H., *Captain and Assistant Surgeon*.—Granted leave of absence for one month, to take effect on or about January 7, 1885 (Madison Barracks, New York).—*S. O. 268, Department of the East*, December 31, 1884.

EWING, CHARLES B., *First Lieutenant and Assistant Surgeon* (Fort Stanton, New Mexico).—Granted leave of absence for two months.—*S. O. 304, A. G. O.*, December 29, 1884.